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ON THE STRENGTH AND OTHER PROPERTIES OF CUBAN WOODS.

AN INVESTIGATION OF THE STRENGTH AND OTHER PROPERTIES OF CUBAN WOODS USED IN ENGINEERING CONSTRUCTION, CONDUCTED IN THE MECHANICAL LABORATORY OF THE DEPARTMENT OF ENGINEERING OF THE STEVENS INSTITUTE OF TECHNOLOGY.

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Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

II.

DETAILS OF TRANSVERSE TESTS.

No. 1. *Caoba* was straight grained, and free from cracks or any other weakening defects. Broke at tension side, midway between the supports. Square rupture. First rupture occurring at 2,300 pounds, second at 1,600, third at 1,950 pounds, fourth at 800 pounds, when the total deflection was 1.25 inches.

No. 2. *Dagame* (J_2). Straight grained, very uniform color, and sound appearance. First rupture took place at 3,450 pounds at tension side, upper side unbroken; very fine, fibrous rupture, and directly under the cross-head. Second rupture at 3,350 pounds. The total number of ruptures were five, at the last of which the hand-wheel had been turned to its lowest position, the sustained load being 150 pounds, and the deflection at this point 7 inches.

No. 3. *Baria* (I_2) broke at tension side diagonally across with the grain. It sustained 1,200 pounds after the first rupture, increasing up to 1,325 pounds, when it broke suddenly in two, the deflection at this point being 1.847 inches.

No. 4. *Majaguilla* (L_2) broke at tension side, upper side compressed. Fibrous rupture. Sustained 3,200 pounds after the first; diminishing gradually in strength, and sustaining 100 pounds when the final deflection was seven inches.

No. 5. *Majagua* (D_1) broke at tension side with a fibrous rupture. After removal of the load the stick recovered remarkably well, a small deflection remaining. Straight grained and of sound condition.

No. 6. *Dagame* (J_2) broke at tension side, fibrous rupture occurring at 2,100 pounds, second at 2,250 pounds, third at 2,280 pounds, fourth at 2,000 pounds, fifth at 2,150 pounds, when the deflection was 4.75 inches. When the deflection was 5.75 inches the load sustained was 1,750 pounds, being held for more than five minutes, after which the load was removed and the set produced was $2\frac{1}{6}$ of an inch, thus showing it to be an excellent

material, very elastic, tenacious, and remarkably strong.

No. 7. *Jiqui* (E_2). First rupture at 3,300 pounds on the tension side, with a long crack. Continued to break gradually.

No. 8. *Jocuma Amarilla* (G_3) broke suddenly in two at 850 pounds. The deflection at the breaking point was 1.65 inches.

No. 9. *Majagua Azul* (D_2) broke first at tension side at 1050 pounds, and midways between the supports. Continued to break gradually, and when the load was 300 pounds the final deflection was 5.75 inches; fibrous fracture.

No. 10. *Ocuje.* First rupture at 1,000 pounds; second rupture at 880 pounds, being long and with the grain.

No. 11. *Cocuyo* (K_1) broke at 2,650 pounds, when the deflection was 1.25 inches. Eight more fractures followed, at the last of which the load sustained was 600 pounds and the deflection 4.25 inches; cross and fibrous rupture.

No. 12. *Baria* (I_2). First rupture at 2,575 pounds on tension side. Broke again at 1,100 pounds, then continued to break gradually at the middle, and with a fibrous rupture.

No. 13. *Yava* (H_2) broke at 1,325 pounds. Broke again at 600 pounds, when the deflection was 1.75 inches. Showed it to be very good in recovering its original form, even after rupture.

No. 14. *Yava* (H_1) broke at tension side at 1,125 pounds, the deflection at this point being 1.5 inches. Broke again at 200 pounds, the deflection then being 3.5 inches. Continued to break gradually. Whenever the load was removed the stick recovered remarkably well.

No. 15. *Guayacancillo* broke at 3,750 pounds, when the deflection was 2.45 inches. Remarkably good specimen, straight grained, and well seasoned. First rupture at tension side.

No. 16. *Caobilla.* First break at tension side at 3,000 pounds, with a deflection of 1.12 inches. Continued to break gradually. Very good specimen, well seasoned and free from knots or other defects.

No. 17. *Sabicu* (A_1) broke with the grain and diagonally across at 2,125 pounds, and a deflection of .875 inches; sudden rupture; good specimen.

No. 18. *Jiqui* (E_1) broke at 2,650 pounds, with a deflection of 1.36 inches. Three large knots in the specimen; one under the cross-head where rupture occurred. Specimen otherwise good.

No. 19. *Majaguilla* (L_2) broke at 2,250 pounds, with a deflection of 1.723 inches. Tension side first; very good and fibrous rupture. Continued to break gradually. Proved to be strong and exceedingly tough material.

No. 20. *Jacuma* (G_1) broke first at tension side at 800 pounds, and a deflection of 1.75 inches. Rupture not fibrous, but quite square and at the middle.

No. 21. *Jucaro Mastelero* (B_2) broke suddenly at a knot two inches from right hand support. Breaking load 1,150 pounds, deflection 2.25 inches. The same piece was tried again, making the distance between the supports equal to 32 inches. Loads were then applied, and it broke at 1,500 pounds, with a deflection of 1.75 inches. Continued to break gradually, and when the sustained load was 100 pounds the deflection was 4 inches.

No. 22. *Sabicu* (A_2) broke at 1,750 pounds, with a deflection of one inch. Sudden rupture, and diagonally across with the grain; good specimen.

No. 23. *Cocuyo* (K_2) broke at 2,300 pounds, the deflection being 1.63 inches. Continued to break very gradually. Good specimen.

No. 24. *Jucaro Prieto* (B_1) broke at 1,675 pounds, with a deflection of 0.85 inches. Continued to break, and at the last break the total deflection was 6 inches. Remarkably tough wood.

No. 25. *Quiebra-hacha* (C_1) broke at 1,375 pounds, with a deflection of 1.32 inches; not fibrous rupture, but square across the grain.

Thus, it may be observed from the preceding, that with the exception of the Sabicu and the Jacuma Amarilla, all the woods tested broke by gradual rupture, the Dagame, Jucara Prieto, Jucaro Mastelero, Majagua and the Majaguilla proving to be the best of all the woods tested, so far as toughness, elasticity and strength are concerned.

The following tables exhibit the data and "Modulus of Rupture," obtained by transverse stress.

They were the same pieces which had previously been used to determine the "Modulus of Elasticity."

The "Modulus of Rupture" was reduced from the formula $R = \frac{3 P l}{2 b d^2}$ in which P represents the breaking load, l the distance between the supports, b and d the breadth and depth of the test-piece respectively.

TRANSVERSE STRESS.

Wood.	Distance between supports in inches.	Breadth in inches.	Depth in inches.	Modulus of Rupture $R=\frac{3}{2}\frac{Pl}{bd^2}$	Modulus of elasticity per formula $E=\frac{Pl^3}{4\triangle bd^3}$	Load producing rupture.	Total deflection in inches.	Weight of a cubic foot in pounds.
Baria (I_1)..	36	1.50	2.00	14850	1794461	1650	1.197	48.6
Baria (I_3).....	36	2.10	2.12	13127	1620000	2575	1.436	48 6
Caoba.....	28	1.72	2.65	8049	1429166	2300	0.399	53.0
Caobilla.....	30	1.46	2 50	15000	1849315	2000	1.120	49.9
Cocuyo (K_1).....	36	2.15	2.04	15900	2310469	2650	1.250	71.7
Cocuyo (K_3).....	40	2.16	2.00	15967	2130000	2300	1.630	71.7
Dagame (J_2).....	40	1.94	2.28	20700	2280820	3450	1.938	56.1
Dagame (J_3).....	40	1.50	2.03	21000	2361620	2100	1.875	56.1
Guayacancillo.....	32	1.50	2.59	18000	1503561	3750	2 450	67.3
Jocuma Am. (G_1)...	30	1.55	1.54	15483	2250000	800	1.750	64.8
Jocuma Am (G_2)....	36	1.47	1.61	16392	2300000	850	1.650	64.8
Jucaro Prieto (B_3)...	28	1.50	1.87	17400	1995636	2175	0.725	67.3
Jucaro Prieto (B_2)...	28	1.45	1.75	15844	2195200	1675	0.825	67.3
Jucaro Mast (B_5)....	38	1.56	1.50	18727	2500000	1150	2.250	55.5
Tiqui (E_2)	40	1.90	2 35	19800	2490272	3300	1.233	74.8
Tiqui (E_1).....	32	1.82	2.00	17451	2520912	2650	1.370	74.8
Majagua (D_1).....	40	1.65	1 88	17436	1939393	1450	2.063	43.9
Majagua (D_3).....	36	1.98	2.12	15000	2000000	2500	1.500	43.9
Majaguilla (L_3).....	36	2.25	2.31	16200	2257548	1600	1.387	69.2
Majaguilla (L_2).....	40	1.66	2.12	18000	2000000	2250	1.723	69.2
Ocuje.....	30	1.38	1.64	12161	1500000	1000	1.250	52.4
Sabicu (A_1).....	24	1.22	1.82	19125	2486070	2125	0.875	59.2
Sabicu (A_5)..... ...	32	1.25	1.76	22000	2400000	1750	1.000	59.2
Qiuebra-hacha (b_1)...	36	1.41	1.75	18562	2100000	1375	1.320	81.1
Yava (H_1).....	40	1.51	1.54	18750	2322463	1125	1.490	54.9
Yava (H_2).....	32	1.50	1.51	18596	2393258	1325	1.500	54.9

TABLES OF DEFLECTIONS AND PERMANENT SETS.

YAVA (H_1).

Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.	Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.
50	0.063	0.063		350	0.435	0.062	
100	0.125	0.062		400	0.498	0.063	0.002
150	0.187	0.062		450	0.560	0.062	
200	0.249	0.062	0.0005	500	0.622	0.062	
250	0.311	0.062		550	0.684	0.062	
300	0.373	0.062		600	0.746	0.062	0.006
650	0 808	0.062		950	1.205	0.075	
700	0.871	63		1000	1.280	0.075	
750	0.936	65		1050	1.370	0.090	
800	0.998	62	0.012	1100	1.490	0.120	
850	1 061	63		1125			
900	1.130	70					

FIQUI (E_2).

Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.	Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.
50	0.013	0.013		750	0.183	0.013	
100	0.027	0.013		800	0.195	0.012	0.003
150	0.037	0.011		850	0.207	0.012	
200	0.049	0.012	0.0005	900	0.220	0.013	
250	0.063	0.014		950	0.233	0.013	
300	0.074	0.011		1000	0.245	0.012	0.005
350	0.085	0.011		1050	0.257	0.012	
400	0.097	0.011	0.001	1100	0.271	0.014	
450	0.110	0.012		1150	0.284	0.013	
500	0.122	0.013		1200	0.297	0.013	0.008
550	0.134	0.012		1250	0.310	0.013	
600	0.146	0.012	0.002	1300	0.323	0.013	0.010
650	0 158	0.012		1350	0.337	0.014	
700	0.170	0.012		1400	0.350	0.013	0.013

FIQUI (E_2)—CONTINUED.

Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.	Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.
1450	0.363	0.013		2400	0.621	0.016	
1500	0.377	0.014		2450	0.637	0.016	
1550	0.389	0.012		2500	0.654	0.017	
1600	0.400	0.011	0.015	2550	0.671	0.017	
1650	0.413	9.013		2600	0.687	0.016	
1700	0.425	0.012		2650	0.703	0.016	
1750	0.438	0.013		2700	0 722	0.019	
1800	0.451	0.013	0.018	2750	0 739	0.017	
1850	0.463	0.012		2800	0.761	0.022	
2900	0.475	0.012		2850	0.782	0.021	
2950	0.488	0.013		2900	0.804	0.022	
2000	0.502	0.014	0.020	2950	0.827	0.023	
2050	0.518	0.014		3000	0 857	0.030	
2100	0.530	0.012		3050	0.888	0.031	
2150	0.545	0.015		3100	0.919	0.031	
2200	0.561	0.016	0.024	3150	0.989	0.070	
2250	0 575	0.014		3200	1.058	0.078	
2300	0.599	0.015		3250	1 143	0.085	
2350	0.605	0.015		3300	1.233	0.090	

MAJAGUILLA (L_2).

Loads in pounds.	Deflection in inches.	Difference in inches.	Set in inches.	Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.
50	0.010	0.010		1450	0.302	0.015	
100	0 019	0.009		1500	0.314	0.012	0.023
150	0.028	0.009		1550	0.325	0.011	
200	0.037	0.009		1600	0 338	0.013	0.026
250	0.045	0.008		1650	0 350	0.012	
300	0.057	0.012	0.001	1700	0 364	0.014	0.040
350	0.065	0.008		1750	0.375	0.011	
400	0.074	0.009	0.002	1800	0.388	0 013	0.045
450	0.085	0.011		1850	0.405	0.017	
500	0.094	0.009	0.003	1900	0.418	0.013	0.050
550	0.103	0.009		1950	0.432	0.014	
600	0 113	0.010	0.004	2000	0.445	0.013	0.053
650	0.123	0.010		2050	0 455	0.010	
700	0 133	0.010	0 005	2100	0.472	0.017	
750	0.143	0.010		2150	0.490	0.018	
800	0.153	0.010	0.006	2200	0.508	0.018	0.067
850	0.163	0.010		2250	0.526	0.018	
900	0.175	0.012	0.009	2300	0.546	0.020	
950	0.188	0.013		2350	0.565	0.019	
1000	0.198	0.010	0.012	2400	0.584	0.019	0.080
1050	0.208	0.010		2450	0.604	0.020	
1100	0.218	0.010	0.014	2500	0.624	0.020	
1150	0.233	0.015		2550	0.645	0.021	
1200	0.243	0.010	0.017	2600	0.663	0.018	
1250	0.253	0.010		2650	0 683	0.020	
1300	0.265	0.012	0.021	2700	0.704	0.021	
1350	0.275	0.010		2750	0.724	0.021	
1400	0.287	0.012	0.022	2800	0.747	0.022	
2850	0.768	0.021		3250	0.990	0.035	
2900	0.790	0.022		3300	1.030	0.040	
2950	0.813	0.023		3350	1.076	0.046	
3000	0.838	0.025		3400	1.126	0.050	
3050	0 863	0.027		3450	1.176	0.050	
3100	0.890	0.031		3500	1.233	0.057	
3150	0.920	0.030		3550	1.293	0.060	
3200	0.955	0 035		3600	1.387	0.084	

(DAGAME T_2).

Loads in pounds.	Deflection in inches.	Difference in inches.	Set in inches.	Loads in pounds.	Deflection in inches.	Difference in inches.	Set in inches.
50	0.016	0.013		1450	0.452	0.016	
100	0.031	0.016	0	1500	0.470	0.018	
150	0.046	0.915		1550	0.485	0.015	
200	0.061	0.015	0	1600	0 506	0.021	0.030
250	0.076	0.015		1650	0.523	0.017	
300	0.091	0.015	0.001	1700	0.540	0.017	
350	0.105	0.014		1750	0.560	0.017	
400	0.120	0.015	0.002	1800	0.576	0.020	
450	0.136	0.016		1850	0.597	0.016	
500	0.151	0.015	0.003	1900	0.613	0.020	0.041
550	0.166	0.015		1950	0.635	0.022	
600	0.181	0.015		2000	0.655	0.020	
650	0.196	0.015		2050	0.678	0.023	
700	0.212	0 016	0.006	2100	0.702	0.024	
750	0 227	0.015		2150	0.725	0.023	
800	0.242	0.015		2200	0.750	0.025	
850	0.258	0.016		2250	0.775	0.025	
900	0.274	0.016	0.009	2300	0.800	0.025	
950	0.290	0.016		2350	0.826	0.026	
1000	0.306	0.016		2400	0.851	0.025	
1050	0.322	0.016		2450	0.879	0.028	
1100	0.338	0.016	0.014	2500	0.906	0 027	
1150	0.354	0.016		2550	0.936	0.030	
1200	0.370	0.016		2600	0.971	0.035	
1250	0.386	0.016		2650	1.006	0.035	
1300	0.402	0.016	0 017	2700	1.046	0.040	
1350	0.419	0.017		2750	1.086	0.040	
1400	0.436	0.017		2800	1.128	0.042	
2850	1.172	0.044		3200	1.549	0.063	
2900	1 217	0 045		3250	1.619	0.070	
2950	1.267	0.050		3300	1.693	0.074	
3000	1.319	0.052		3350	1.773	0.080	
3050	1.371	0.052		3400	1.858	0.085	
3100	1.426	0.055		3450	1.938	0.080	
3150	1.486	0.060					

BARIA (I_2).

Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.	Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.
50	0.027	0.027		900	0.486	0.029	
100	0.054	0.027		950	0.515	0.029	
150	0.081	0.027		1000	0 545	0.030	0.018
200	0.108	0.027	0.0015	1050	0.575	0.030	
250	0.133	0.025		1100	0.615	0.040	
300	0 159	0.026	0.003	1150	0.650	0.035	
350	0.184	0.025		1200	0.683	0.033	0.037
400	0.210	0.026	0.004	1250	0.725	0.048	
450	0.239	0.029		1300	0.780	0.055	
500	0.265	0.026	0.006	1350	0.820	0.040	
550	0.290	0.025		1400	0.862	0.042	0.068
600	0.316	0.026	0.008	1450	0.922	0.060	
650	0.345	0.029		1500	0.987	0.065	
700	0.371	0.026	0.009	1550	1.057	0.070	
750	0.400	0.029		1600	1.127	0.070	
800	0.427	0.027	0.010	1650	1.197	0.070	
850	0.457	0.030					

JUCARO PRIETO (B_2).

Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.	Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.
50	0.016	0.016		900	0.325	0.020	0.020
100	0.032	0.016		950	0.345	0.020	
150	0.050	0.018		1000	0.365	0.020	0.026
200	0.068	0.018	0.001	1050	0.388	0.023	
250	0.086	0.018		1100	0.414	0.026	0.035
300	0.102	0 016	0.002	1150	0.442	0.028	
350	0.119	0.017		1200	0.470	0.028	0.044
400	0.137	0.018	0.003	1250	0.500	0.030	
450	0.154	0.017		1300	0.530	0.030	0.060
500	0.172	0.018	0.005	1350	0.560	0.030	
550	0.189	0.017		1400	0.600	0.040	0.084
600	0.207	0.018	0.007	1450	0.640	0.040	
650	0.226	0.019		1500	0.680	0.040	0.104
700	0.245	0.019	0.011	1550	0.725	0.045	
750	0.265	0.020		1600	0.775	0.050	0.145
800	0.285	0.020	0.015	1650	0.825	0.050	
850	0.305	0.020		1675			

MAJAGUA AZUL (D_1).

Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.	Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.
50	0 038	0.038		800	0.629	0.045	
100	0.075	0.037		850	0.678	0.049	
150	0.113	0.038		900	0.729	0.049	0.031
200	0.150	0.037	0.002	950	0.790	0.063	
250	0.187	0.037		1000	0.840	0.050	
300	0.225	0.038		1050	0.900	0.060	
350	0.263	0.038		1100	0.965	0.065	
400	0.301	0.038	0.006	1150	1.050	0.085	
450	0.340	0.039		1200	1.133	0.083	
500	0.379	0.039		1250	1.243	0.110	
550	0.418	0.039		1300	1.373	0.130	
600	0.458	0.040	0.013	1350	1.513	0.140	
650	0.498	0.040		1400	1.713	0 200	
700	9.540	0.042		1450	2.067	0.350	
750	0.584	0.044					

CAOBA.

Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.	Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.
50	0.006	0.006		1200	0.165	0.010	0.009
100	0.012	0.006		1250	0.174	0.009	
150	0.020	0.008		1300	0.183	0.009	
200	0.026	0.006	0.0005	1350	0.191	0.008	
250	0.032	0.006		1400	0.198	0.007	0.017
300	0.038	0.006		1450	0.205	0 007	
350	0.045	0.006		1500	0.217	0.008	
400	0.051	0.007	0.001	1550	0.222	0.009	
450	0.057	0.006		1600	0.230	0.008	0.021
500	0.063	0.006	0.002	1650	0.240	0.010	
550	0.069	0.006		1700	0.250	0.010	
600	0.077	0.008	0 003	1750	0.260	0.010	
650	0.084	0.007		1800	0.270	0.010	
700	0.091	0.007	0.004	1850	0.280	0.010	
750	0.098	0.007		1900	0.290	0.010	
800	0.105	0.007	0.005	1950	0.300	0.010	
850	0.107	0.007		2000	0.310	0.010	0.035
900	0.119	0.007	0.006	2050	0.325	0.015	
950	0.126	0.007		2100	0.340	0.015	
1000	0.133	0.007	0.007	2150	0.350	0.010	
1050	0.140	0.007		2200	0.360	0 010	0.049
1100	0.148	0.008		2250	0.375	0.015	
1150	0.155	0.007	0.009	2300	0.394	0.019	

COCUYO (K_2).

Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.	Load in pounds.	Deflection in inches.	Difference in inches.	Set in inches.
50	0.021	0.021		1200	0.535	0.030	0.025
100	0.042	0.021	0	1250	0.565	0.030	
150	0.063	0.021		1300	0.595	0.030	
200	0.084	0.021	0	1350	0.625	0.030	
250	0.105	0.021		1400	0.650	0.025	
300	0.126	0.021	0.003	1450	0.680	0.030	
350	0.147	0.021		1500	0.710	0.040	0.036
400	0.169	0.021	0.004	1550	0.750	0.040	
450	0.190	0.021		1600	0.790	0 040	
500	0.211	0.021	0.305	1650	0.830	0 040	
550	0.233	0.022		1700	0.870	0.040	
600	0.256	0.022	0.006	1750	0.910	0.040	
650	0.278	0.022		1800	0.950	0.040	
700	0.299	0.021	0.008	1850	0.990	0.040	
750	0.320	0.021		1900	1.040	0.050	
800	0.344	0.024	0.011	1950	1.090	0.050	
850	0.366	0.022		2000	1.150	0.060	
900	9.390	0.024		2050	1.210	0.060	
950	0 414	0.024		2100	1.270	0.060	
1000	0.434	0.020	0.016	2150	1.340	0.070	
1050	0.458	0.024		2200	1.420	0.080	
1100	0.482	0.024		2250	1.510	0.090	
1150	0.505	0.023		2300	1.630	1.020	

Experiments by Tension.

These experiments, as already stated, are somewhat unsatisfactory. It is clear that rupture in many instances did not occur by tension, but rather by shearing or splitting, which always gives a much lower result. The manner of breaking is illustrated in the last column of the table, page 449:

Experiments by Compressive Stress.

The modulus of rupture for crushing was deduced from the formula $C=\frac{P}{K}$, in which P represents the crushing load, and k the sectional area of the test-piece, which in this case equals πr^2.

TESTS BY COMPRESSION.

WOOD.	Number of specimen.	Length in inches.	Diameter in inches.	Crushing loads in pounds.	Modulus of rupture per formula $C=\frac{P}{K}$	Mean value for C.
Baria	1	8.00	1.75	21500	9472	9472
Caoba	2	7.50	1.50	9500	5397	5397
Caobilla	3	6.25	1.25	11500	10087	10087
Cocuyo	4	8.00	1.75	21500	9472	9757
Cocuyo	5	8.00	1.75	22800	10043	
Dagame	6	8 00	1.75	26500	11674	11092.5
Dagame	7	7.50	1.50	18500	10511	
Guayacancillo	8	5.00	1.25	11500	10087	10262
Guayacancillo	9	5.00	1.25	11900	10438	
Jocuma Am	10	6.00	1.25	13000	11403	
Jocuma Am	11	6.00	1.25	12500	10973	10967
Jocuma Am	12	6.00	1.25	12000	10522	
Jucaro Prieto	13	7.00	1.30	17000	12883	
Jucaro Prieto	14	6.00	1.30	17000	12883	
Jucaro Prieto	15	5.00	1.30	17500	13258	13158
Jucaro Prieto	16	4.00	1.30	17500	13258	
Jucaro Prieto	17	3.00	1.30	18000	13600	
Jucaro Mastelero	18	6.00	1.25	12500	10973	10973
Jucaro Mastelero	19	6.00	1.25	12500	10973	
Jiqui Comun	20	8.00	1.75	32000	14096	
Jiqui "	21	7.00	1.50	21000	11931	
Jiqui "	22	2.50	1.25	12200	10701	12314
Jiqui "	23	8.00	1.75	29500	13000	
Jiqui "	24	7.00	1.25	13500	11843	
Majagua Azul	25	7.50	1.50	14500	8238	8504
Majagua Azul	26	6.00	1.25	10000	8771	
Majaguilla	27	5.00	1.50	15250	8664	
Majaguilla	28	2 50	1.50	16500	9378	9044
Majaguilla	29	7.00	1.50	16000	9090	
Ocuje	30	6.00	1.25	9900	8684	8684
Quiebra-hacha	31	5.00	1.25	16500	14473	13596
Quiebra-hacha	32	5.00	1.25	14500	12719	
Sabicu	33	6.00	1.25	11500	10087	10043
Sabicu	34	6.00	1 27	12500	10000	
Yava	35	7 50	1.50	20800	11818	
Yava	36	5 00	1.25	14500	12719	12618
Yava	37	6.00	1.25	14500	12719	

EXPERIMENTS BY TORSIONAL STRESS.

The test-pieces used in these experiments were carefully turned in an engine lathe, and great care was taken to secure uniformity of size.

In these tests the pendulum bob was removed from the machine, and it was therefore necessary to standardize the machine in order to ascertain the modifications made by this change.

It was found that the maximum moment of the pendulum arm, *i. e.*, its moment when in a horizontal position, was equal to 24 pounds multiplied by 4.17 feet, which is the distance from the axis of rotation to the bolt hole. The maximum height of pencil when the arm was horizontal was 4.95 inches.

Therefore $(24\times4.17)\div4.95=20$ will give the stress in foot pounds for each inch of ordinate, on the diagram. Each inch of abscisa equals 10° of twist. The resistance to friction was found to be very small, and may be taken as $\frac{1}{2}$ foot-pound.

The modulus of torsional elasticity is found from the formula $G=\frac{PAl}{\theta I_p}$ in which P represents the twisting force in foot pounds, A the arm of P, l the length of neck of test-piece in inches, θ the angle of torsion within the elastic limit, and I_p the polar moment of inertia, which, for a rectangular section, equals $\frac{1}{2}\pi r^4$.

TENSION TESTS.

Wood.	Laboratory number.	Breaking loads in pounds.	Modulus of rupt'e per form'a $T=\frac{P}{K}$	Kind of rupture.
Baria	1	4000	8000	Irregular.
Caoba	2	5200	10400	Square break
Caobilla	3	5600	11200	Irregular.
Cocuyo	4	6000	12000	Detrusive,
Dagame	5	6500	13000	Irregular.
Guayacancillo.	6	8000	16000	"
Jocuma	7	1500	3000	Detrusive.
Jucaro Prieto	8	7600	15200	Square.
Jucaro Mast'lero	9	6000	12000	"
Jiqui Comun	10	7500	15000	Detrusive.
Majagua Azul	11	5800	11600	Irregular.
Majaguilla	12	6000	12000	Nearly sq'are
Ocuje	13	2000	4000	Detrusive.
Sabicu	14	7000	14000	Irregular.
Quiebrahacha	15	5600	11200	Nearly sq'are
Yava	16	5000	10200	" "

TORSION TESTS.

Wood.	Angle.		Stress in torsion foot pounds.		Area in square inches.	
	At elastic limit.	Final.	At elastic limit.	At maximum ordinate.	Up to the elastic limit.	Total
Baria	5°	237°	16	18	.40	
Caoba	10°	305°	12	12	.40	
Caobilla	8°	115°	14	14	.45	
Cocuyo	14°	260°	12	16	.70	
Dagame	15°	341°	16	26	.90	
Guayacancillo.	6°	254°	16	18	.35	
Jocuma	8°	172°	16	16	.40	
Jucaro Prieto	7°	281°	20	20	.45	
Jucaro Mastelero	10°	290°	18	20	.60	
Jiqui Comun	9°	280°	22	22	.85	
Majagua	3°	348°	12	12	.15	
Majaguilla	11°	305°	16	18	.80	
Ocuje	11°	220°	16	16	.50	
Sabicu	7°	320°	18	18	.40	
Quiebra-hacha	8°	235°	26	26	.50	
Yava	6°	235°	12	14	.20	

CONCLUSIONS.

It appears from this investigation that the native woods of Cuba, of which the specimens experimented upon are good representatives, possess qualities which render them superior in every respect to the hard woods of the United States or Europe, not only on account of their greater hardness, closer grain, finer texture, and great durability, but also owing to their greater elasticity and strength, in which respects they are far better, as a rule, than even ash or oak. Their moduli of elasticity vary from 1,500,000 to 2,500,000, and is generally somewhat above 2,000,000. The modulus of rupture varies from 10,000 to 20,000, and averages about 18,000.

THE PROPORTIONS OF ARCHES, DERIVED FROM FRENCH PRACTICE.

Written for VAN NOSTRAND'S MAGAZINE, by E. SHERMAN GOULD, C. E.

CONSIDERED mathematically, the designing and calculating of arches form one of the most intricate and involved problems of applied mechanics; a fact abundantly proven by the numerous and conflicting theories of the arch which have been advanced, from time to time, by various and eminent mathematicians. Considered practically, however, the problem, at least so far as regards design, is greatly simplified by the many examples furnished by existing structures of all varieties and dimensions, which afford incontrovertible demonstration of the correctness of their proportions by safely fulfilling their intended duties, to say nothing of that other large class of non-existing structures which furnish the negative instruction to be derived from failure.

The purpose of this paper is to present as careful a digest as I have been able to prepare, of the actual practice of French engineers in regard to the proportioning of arches. The authorities which I have mainly used are: "*Routine de l'etablissment des routes*," by Dejardin, a work much quoted by French writers. "*Pratique de l'art de construire.*" by Claudel and Laroque, and a most excellent and

very recent volume, already noticed in this magazine, by Dubosque, on the theory and practice of the building of retaining walls, bridges and viaducts. The last two of these are themselves digests of the writings of many standard French authors. I may add, that these works were written by practical men, themselves engineers, or constructed and connected with many important public works in France.

In designing an arch, the first step is to determine the proper depth of keystone or thickness at the crown. The great Perronnet, of whose genius the magnificent Bridge of Neuilly is but one example among many, deduced from his own observations and practice a formula which, reduced to English measures, may be best written thus:

$$e=1+0.035\,S.$$

In this equation e=thickness of arch at crown in feet, S=span in feet.

A modern writer, Monsieur Lêveillé, *ingenieur en chef des ponts et chaussees*, has, from a careful study of the dimensions of existing structures, declared this formula to be of general application, and suited to arches of all forms, whether semi-circular, segmental, elliptical or false-elliptical, and even to railroad bridges and arches sustaining heavy surcharges of earth.

Another modern 1854 author, Monsieur Dejardin, *injenieur des ponts et chaussees*, whose death at an early age did not prevent him from connecting his name with more than one important structure, and who has left as a legacy to the profession the admirable practical treatise on arches named above, gives the following series of formulæ:

For a semi-circular arch,

$$e=1+0.1\,R$$

In which R=radius of the intrados.

For segmental arches, putting S=span, and V=ver-sine or rise, then;

when $\frac{v}{s}=\frac{1}{6}$; $e=1+0.05\,R$

" $\frac{v}{s}=\frac{1}{8}$; $e=1+0.035\,R$

" $\frac{v}{s}=\frac{1}{10}$; $e=1+0.02\,R.$

For elliptical, or false-elliptical, or "basket-handled" arches

when $\frac{v}{s}=\frac{1}{3}$; $e=1+0.07\,R,$

in which R equals the radius of curvature at the crown. In the case of the ellipse, R equals the minor axis.

Another approved authority, Monsieur Croizette,-Desnoyers, gives for semi-circular arches, and also segmental, etc., arches, when $\frac{v}{s}$ is greater than $\frac{1}{6}$, the general formula;

$$e=0.50+0.28\sqrt{2\,R}.$$

In the case of elliptical, or false-elliptical arches, R equals the radius of a fictitious arc of a circle, having a ver-sine equal to the rise of the arch under consideration.

In the case of segmental arches of low ver-sine and wide span, Monsieur Croizette-Desnoyers consider that the coefficient of the radical may be proportionally diminished, thus:

when $\frac{v}{s}=\frac{1}{6}$; $e=0.50+0.26\sqrt{2\,R}$

" $\frac{v}{s}=\frac{1}{8}$; $e=0.50+0.24\sqrt{2\,R}$

" $\frac{v}{s}=\frac{1}{10}$; $e=0.50+0.22\sqrt{2\,R}$

" $\frac{v}{s}=\frac{1}{12}$; $e=0.50+0.20\sqrt{2\,R}$

It will be noticed that in none of the above formulæ does the character of the material used enter as a factor. Now it is evident that an arch built of granite voussoirs dressed to quarter-inch joints and laid up in Portland cement—to take a rather extreme case—would admit of much lighter dimensions than an ordinary brick arch laid up in common lime mortar.

To meet the case of exceptionally good materials and workmanship, another author, Monsieur Dupint, gives, for semi-circular arches,

$$e=0.37\sqrt{s}$$

For segmental, etc.

$$e=0.28\sqrt{s}$$

in which s=span. In all the above formulas the dimensions are always in feet.

Among French practiciens, the formulæ of Dejardin seem to have received the most general acceptance for arches of moderate span. They give in many cases thicknesses which American engineers might consider excessive. It must be borne in mind, however, that the French quarries furnish a great deal of building-stone weighing only 125 lbs. and under, per cubic foot, with a resistance to crushing of but from 4,000 to 6,000 lbs. per square inch.

For general practice I should feel disposed to adapt Perronnet's formula.

(No. 1) $e=1+0.035\,s$

Not only on account of its simplicity, which is a small matter, for in the case of an important structure like a bridge one might well afford a few moments' additional time in working out a more complicated formula if there were anything to be gained by it, but because the results it gives tally so well with many existing structures. In substantiation of this I append a most instructive table, giving the dimensions, real and calculated, of a large number of bridges. It will be seen that the last two columns contain the thickness of the abutments. The calculated thickness of these is obtained by a formula given by Monsieur Léveillé, of which mention and use will be made later on. Two of the terms of this formula contain e, or the thickness of the crown. It is the calculated value of e that is used. The height of roadway above the extrados of the arch also enters one term of the formula. This is assumed at 2 feet in calculating the last column.

I have added the dimensions of two English bridges; that of Chester, and that carrying the Great Western Railway over the Thames at Maidenhead, not given in the original table. I have also added a column giving the thickness at the crown or depth of keystone according to Dupuit's formulæ

(*See Table on following page.*)

In the column of calculated thickness, in the above table, slight discrepancies in the decimals may be perceived from the fact that in most cases the meters of the original table were simply reduced to feet by multiplying by 3.3, and also because Perronnet's formula, as used by Léveillé, is modified by him, for the sake of round numbers, so as to give results very slightly less than in the form which I have given, and which is nearer the original.

I think the general agreement of results obtained by this formula with the dimensions of a number of wide-span railroad bridges of tested security, as given by the above table, justifies confidence in it. Dupuit's formula is found to agree very well for semicircular arches of the comparatively short spans given in the table, while the divergence is quite wide in the other classes. It is noteworthy that his formula exactly checks the depth of keystone of the Chester bridge, which is perhaps the boldest of its kind extant.

Although Monsieur Léveillé considers his formula as giving a sufficient depth of keystone either for carrying heavy railroad traffic over a roadbed raised two feet above the extrados of the crown, or supporting a heavy surcharge of earth, it is proper to remark that other engineers consider it necessary to introduce a variable term in the case of an extra heavy surcharge, whether of earth or any other load. If it is considered necessary to augment the depth of keystone in such cases, we may borrow an expression from this variable term, from the German and Russian practice, and write our formula thus:

$$e=1+0.035+0.02\,\mathrm{H}.$$

H=height in feet of the earth embankment over the extrados at the crown. If the surcharge be of any other kind of loading, its weight is to be reduced to an equivalent height of earth embankment. This term can be added to any one of the formulæ used for determining the value of e.

As regards the question of surcharging an arch, it must be borne in mind that a great difference exists between a surcharge, properly so called, consisting of a high bank of earth, or other stationery and inert material, and the "live load" occasioned by, for instance, the passage of heavy trains. In the former case, the surcharge nearly increased the dead load, more or less evenly distributed over the arch; and while, up to a certain point, it augments the crushing pressure upon the arch sustaining it, on the other hand, *if there is enough of it and the abutments are sufficiently solid*, it effectually pre-

Designation of Bridge.	Span.	Rise.	Depth of keystone.			Height of abutment.	Thickness of abutment.	
			Act'al	Calculated.	Dupuit's formula.		Act'al	Calculated.
	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.
1° *Segmental Arches.*								
Des Fruitiers, chemin de fer du Nord..	13.20	2.31	1.81	1.55	1.00	13.20	5.94	5.97
De Paisia	16.50	2.64	1.72	1.65	1.10	6.60	5.61	6.43
De Méry, chemin de fer du Nord.....	25.17	2.97	2.14	1.94	1.40	14.20	11.71	11.88
De Mélisey	37.60	4.95	1.98	2.34	1.70	11.71	17.61	15.44
De Couturette, at Arbois	42.90	6.13	2.97	2.54	1.80	6.60	17.16	13.95
Over the Salat	46.10	6.27	3.63	2.64	1.90	20.49	19.14	20.00
De la rue des Abattoirs, at Paris, chemin de fer de Strasbourg..	52.90	5.11	2.97	2.87	2.00	12.96	33.00	23.89
Over the Forth, at Stirling	53.50	10.25	2.75	2.90	2.00	20.75	16.00	16.90
Saint-Maxence, over the Oise	77.20	6.40	4.80	3.65	2.50	27.85	38.94	40.10
Over the Oise, chemin de fer du Nord.	82.70	11.75	4.60	3.85	2.60	17.90	31.65	30.75
De Dorlaston	87.00	13.50	3.50	3.95	2.60	16.55	32.20	29.7
Grosvenor Bridge, Chester	200.00	42.00	4.00	8.00	4.00			
2° *Semi-Circular Arches.*								
Du crochet, chemin de fer de Paris à chartres	13.20		1.65	1.55	1.30	13.20	4.95	5.30
De Long-Sauts, chemin de fer de Paris à chartres	16.50		1.81	1.65	1.50	9.90	5.90	5.85
D'Enghien, chemin de fer du Nord...	24.40		1.95	1.90	1.80	6.60	6.93	7.15
De Pantin, canal St. Martin	27.00		2.47	2.00	1.90	11.85	10.55	8.16
De la Bastille, canal St. Martin	36.30		3.95	2.20	2.20	20.75	9.90	10.70
Des Basses-Granges, Orléans à Tours.	49.40		3.95	2.73	2.60	6.60	12.50	11.55
3° *Elliptical, or false-elliptical arches.*								
De Charolles	19.80	7.55	1.95	1.75	1.25	1.30	5.25	5.25
Du Canal Saint-Denis	39.50	14.85	2.95	2.40	1.80	10.20	12.35	11.20
De Chateau-Thierry	51.30	17.10	3.75	2.75	2.00	13.65	15.00	13.90
De Dôle, over the Doubs	52.40	17.50	3.75	2.80	2.00	1.35	11.85	12.85
Wellesley, at Limeric	70.00	17.50	2.00	3.45	2.30	12.00	16.50	21.30
D'Orléans, chemin de fer de Vierzon.	79.50	26.30	3.95	3.75	2.50	2.85	18.40	17.55
De Trilport	80.70	27.80	4.45	3.75	2.50	6.40	19.30	20.40
De Mantes, over the Seine	115.20	34.60	6.40	5.00	3.00	3.20	28.90	28.50
De Neuilly, over the Seine	128.00	32.00	5.35	5.35	3.20	7.55	35 50	35.50
Maidenhead, Great Western Railway.	128.00	24.25	5.25	5.45	3.20			

vents the arch from yielding in any way save by direct crushing of the material of which it is composed. In this manner, under fitting conditions, heavy surcharging becomes an element of strength to an arch, neutralizing as it does the sometimes dangerous effects of a variable load, like that of a passing train which brings unbalanced strains upon the structure. Indeed, Captain Woodbury in his treatise upon the arch, maintains the proposition "that an arch impossible or impracticable without surcharge, may become perfectly safe when a load of sufficient depth has been added."

In a correctly proportioned arch, the thickness should increase from the keystone towards the haunches. Before showing how the proper increase is arrived at, I will give the formulæ by which the thickness of the abutments is obtained.

Monsieur Léveillé gives the following series of formula for the thickness of the abutments of railroad bridges, carrying

a horizontal surcharge of ballast raised two feet above the extrados of the key.

(No. 2) Semicircular arches; $E = (2+0.162S)\sqrt{\frac{h+0.25S}{H} \times \frac{0.865S}{0.25S+e}}$

(No. 3) Segmental arches; $E = (1+0.212S)\sqrt{\frac{h}{H} \times \frac{s}{f+e}}$

(No. 4) Elliptical and false elliptical; $E = (1.41+0.154S)\sqrt{\frac{h+0.54f}{H} \times \frac{0.84S}{0.465f+e}}$

E = thickness of abutment below springing line.

s = span.

h = height of abutment, or distance between springing line and top of foundation.

e = depth of keystone.

f = rise. In a full-centered arch $f = R$.

H = vertical distance between top of roadway and top of foundations. It will be equal to $h+f+e+2$.

It is from these formulæ that the thickness of the abutments in the last column of the above table has been calculated. According to Monsieur L., it is not neccesary to augment the dimensions obtained by his formulæ for heavy surcharges. The best justification of his formulæ is the comparison shown by the table.

There is a much simpler formula, which I have adapted from German and Russian practice, which gives, for semicircular arches, values very near those given by Léveillé's. It is

(No. 5) $E = 1+0.04\ (5s+4h)$

We will now examine the principles which govern the proportions of the other parts of an arch.

It is a matter of common observation that a semicircular arch can be carried up to a certain height above the springing line, without the support of centering. This height varies, of course, according to the nature of the materials used and the character of the workmanship. As a general rule, however, and probably in most cases well within the truth, we may take this height at half the rise of the arch, that is, half the radius with which the intrados is struck. Expressed in angular distance, this height corresponds with an angle of 30° from the horizontal, or 60° from the vertical. Owing to the self-supporting character of the arch below this point, the joint situated 30° above the springing line is called the *joint of rupture*. We may, therefore, consider the central portion, situated between the two joints of rupture, as the arch proper; all that portion lying below the joint of rupture on each side being regarded as forming part of the abutment. As the total angular amplitude of a full-center arch is 180°, the deduction of 30° from eacn side leaves a remainder of 120°. Dejardin says, therefore, "*There can be no arch beyond* 120°."

This method of finding the position of the joint of rupture by placing it at half the rise of the arch, is general; that is, it applies to segmental, elliptical and "basket-handled" arches as well as semicircular ones. In the case of a segmental arch of which the angular amplitude is equal to, or less than 120°, the joint of rupture is, of course, situated at the springing line. If its amplitude is greater than 120°, we imagine the full-center arch of same radius to be struck, and measure down from the crown of the intrados, a distance equal to half the radius. A horizontal line drawn through this point will then cut the curve of the intrados at the joint of rupture. Or, we may lay off 60° from the vertical passing through the crown of the arch and obtain the desired point in that way.

For elliptical and false-elliptical arches, the joint of rupture is also found at half the rise. Should the arch be incomplete, *i.e.*, if it is not carried down until the springing line is horizontal, it must be completed upon the drawing, and half the rise of the completed arch measured down from the crown. A horizontal line drawn through the point thus determined will, by its intersection with the intrados, indicate the position of the joint of rupture. It is very rarely, however, that incomplete elliptical arches are met with.

It is evident that in order to preserve an uniform resistance in all parts of the arch that its thickness should increase from the crown to the joint of rupture. There is a very convenient rule for proportioning this increase, which is, simply that *the length of each joint, between the*

joint of rupture and the crown, measured radically, should be such that its vertical projection is equal to the depth of keystone. Expressed as a formula, this rule would be stated thus:

$$l' = \frac{e}{\sin a'}$$

in which l'=the length, measured radially, of any joint, a'=the angle which such joint makes with the horizontal, and e=depth of keystone.

This rule enables us to at once determine the *length of the joint of rupture*, which we will designate by l. It can always be expressed in terms of e, that is, by the depth of keystone multiplied by a certain coefficient. Thus, we have for full centered arches, or segmental arches, when $\frac{v}{s}$ is equal to or greater than $\frac{1}{4}$,

$$l = 2e. \qquad \text{(No. 6)}$$

For flatter segmental arches, Monsieur Croizette-Desnoyers has calculated the following series of coefficients:

No. 7

For	$\frac{v}{s}=\frac{1}{6}$;	$l=1.40\,e$ . .	(a)
"	$\frac{v}{s}=\frac{1}{8}$;	$l=124\,e$ . .	(b)
"	$\frac{v}{s}=\frac{1}{10}$	$l=1.15\,e$ . .	(c)
"	$\frac{v}{s}=\frac{1}{12}$	$l=1.10\,e$ . .	(d)

For other ratios of $\frac{v}{s}$, the value of l may be determined by interpolation.

Monsieur Desnoyers also gives, for elliptical and "basket-handled" arches,

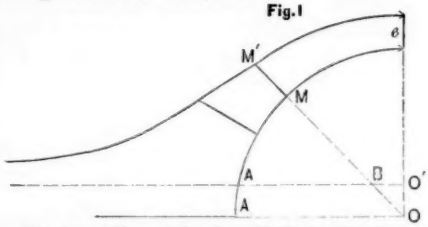
No. 8

For	$\frac{v}{s}=\frac{1}{3}$;	$l=1.80\,e$ . .	(a)
"	$\frac{v}{s}=\frac{1}{4}$;	$l=1.60\,e$ . .	(b)
"	$\frac{v}{s}=\frac{1}{5}$;	$l=1.40\,e$ . .	(c)

Having the depths of keystone and the length of the joints of rupture, it is now only necessary to connect their extremities in order to attain the curve of the extrados. To do this, and conform to the formula $l'=\frac{e}{\sin a'}$, Dejardin gives the following graphical method, for full-centered or segmental arches:

Let AO, Fig. 1, be the springing line of a full-centered arch. Draw the horizontal line O′A′ distant O′O=e from AO. Then the length of any joint MM′ is obtained by drawing OM produced, and making BM′=OM=R. This construction may be continued below the joint of rupture MM′, as is sometimes desirable in the case of contiguous arches. Below this joint the length rapidly increases, and finally an asymptote to the springing line produced is obtained.

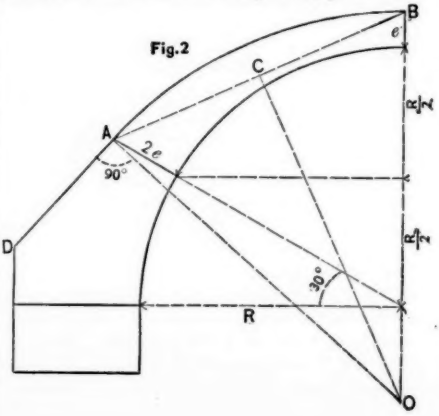
Fig. 1



We have the authority of Dejardin for stating, that *if the extrados of the arch proper, i.e., that portion of it comprised between the two joints of rupture, be drawn subject to the condition* $l'=\frac{e}{\sin a'}$, *the depth of keystone* e *being properly proportioned, the arch itself conforms strictly to the requirements of stability, and no further calculation is necessary to determine the fact.*

Monsieur Dubosque gives a more rapid method than that of Dejardin, just de-

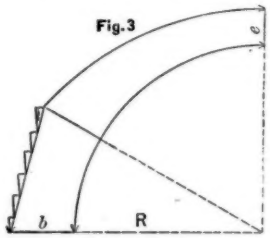
Fig. 2



scribed, of striking the curve of the extrados between the joints of rupture, as follows: Join the outside extremity of the joint at the crown, and of the joint of rupture by the line AB (Fig. 2). Bisect AB in C. Draw CO perpendicular to AB, cutting the vertical BO in O. With the radius AO=BO, describe the arc AB, forming the required extrados. This method gives an intermediate thickness somewhat greater than does that of Dejardin.

Dubosque then finishes the exterior lines of his arch by drawing a tangent AD (Fig. 2) to the curve of the extrados, and producing it until it intersects at D with the back of the abutment. This construction is applicable to all kinds of raches, full-centered, segmental, etc.

Dejardin considers a full-centered arch as composed of three separate parts, namely the arch proper comprised between the two joints of rupture; an intermediate portion on each side of the center comprised between the joints of rupture and the springing line, and finally the abutments proper, comprising those portions of the structure lying between the springing lines and the top of the foundations.



The first and third of these parts we have already considered. The intermediate parts are determined by Dejardin, by simply taking a distance b on the springing line (Fig. 3) equal to $\frac{1}{4}$ of the sum of the radius and twice the depth of keystone. Thus,

$$b=\frac{R+2e}{4}.$$

It should be understood, of course, that in this calculation e is determined by Dejardin's formula.

The extremity of the joint of rupture and of b are then joined by either a straight line, or offsets exterior to a straight line (Fig. 3). Should the radius be so small (less than 5 feet) that b thus determined is less than the horizontal projection of the joint of rupture, a perpendicular is dropped from its extremity upon the springing line.

I have given this method of Dejardin, simply to show how far, in the judgment of an approved authority, this portion of the arch may, with perfect safety, be reduced, as in special cases such reduction might be desirable. I think, however, that Dubosque's method, given above, will generally be preferred.

In order to exemplify the preceding rules, I will now give designs for four different arches, respectively, semi-circular, segmental, elliptical, and "basket-handled," being single-span railroad bridges, the level roadbed being in all cases 2 feet above the extrados of the arch at the crown.

First. A full-centered arch (Fig. 4), 50 feet span; abuments six feet high from top of foundation to springing line.

We have three dimensions only to determine by formula, viz., the depth of keystone, the thickness of the abutments, and the length of the joint of rupture. All the other dimensions will be determined by a purely graphical construction.

Substituting the given values in formulæ (1) and (2) we have,

$$e=1+0.035\times 50=2.75 \text{ ft.}$$

$$E=(2+0.162\times 50)\sqrt{\left(\frac{6+0.25\times 50}{6+25+2.75+2}\right)\times\left(\frac{0.865\times 50}{0.25\times 50+2.75}\right)}=(10.1)\sqrt{0.517\times 2.84}$$

$$=12.22 \text{ ft., say } 12.25 \text{ ft.}$$

Or, using the simplified formula No. 5,

$$E=1+0.04(5s+4h)=11.96 \text{ ft.}$$

which serves as a very good check upon the more complicated formula.

For the length of the joint of rupture we have, by substituting the value of e in formula (6),

$$l=2\times 2.75=5.5 \text{ ft.}$$

The curve of the extrados between the joint of rupture and the crown is struck in, as described above, and the tangent produced to its point of intersection with the back of the abutment produced, as also described, and the arch is complete. All the principal dimensions are given in the figure (Fig. 4).

Fig.4

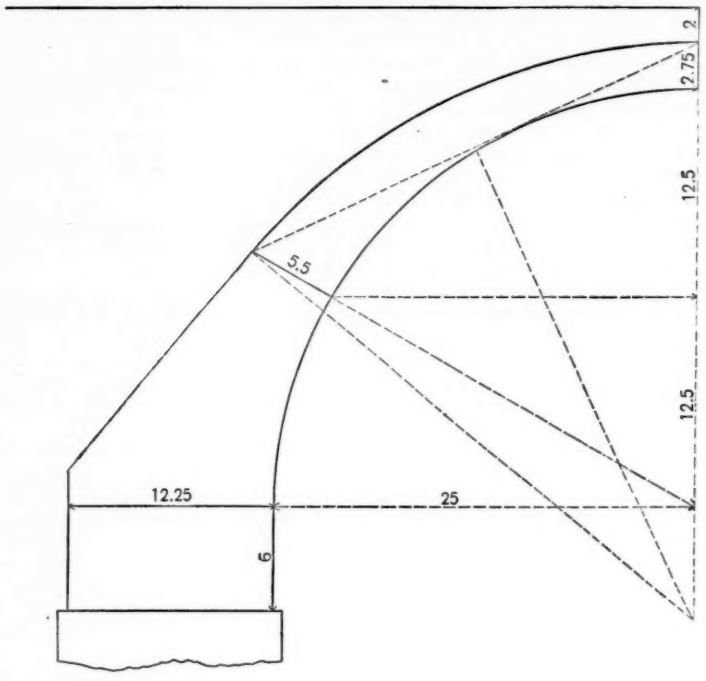
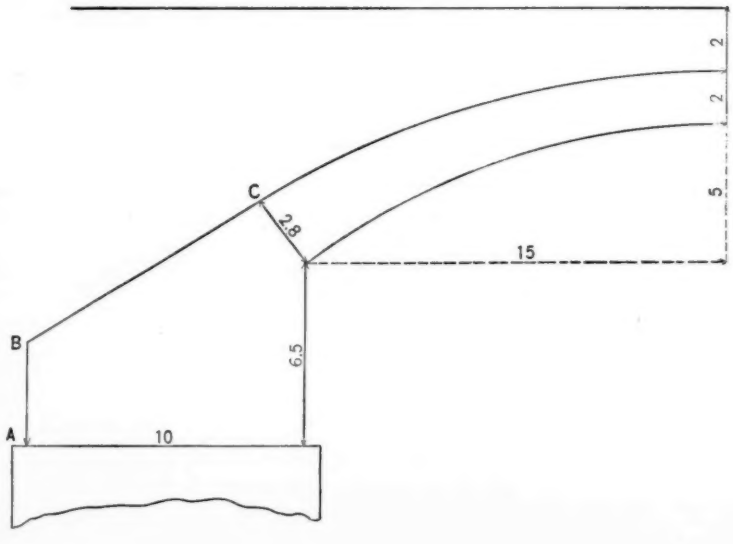


Fig.5



Second. A segmental arch, 30 feet span, 5 feet rise; height of abutments, 6.5 feet (Fig. 5). Using formulæ (1) and (3) we have,

$$e=1+0.035\times30=2.05 \text{ ft., say 2 ft.}$$

$$E=(1+0.212\times30)\sqrt{\left(\frac{6.5}{6.5+5+2+2}\right)\left(\frac{30}{5+2}\right)}$$

$$=7.36\sqrt{0.42\times4.3}=9.86 \text{ ft, say 10 ft.}$$

For the length of the joint of rupture, as $\frac{v}{s}=\frac{1}{6}$ we use formula (*a*) No. 7, which gives,

$$l=1.4\times2=2.8 \text{ ft.}$$

As the arch in question is of less amplitude than 120°, that is, $\frac{v}{s}$ being smaller than $\frac{1}{4}$, the joint of rupture occurs at the springing line.

The curve of the extrados between the crown and the joint of rupture is struck in the same way as already described for a full-centered arch, and the outline is completed as for a full-centered arch, by producing the tangent of this curve beyond the joint of rupture until it intersects with the vertical back of the abutment.

Monsieur Dubosque remarks, in speaking of this class of arch, that the height AB should never be less than from 3 to 5 feet, so as to insure a square pressure against the bank. Should the tangent from the extremity of the joint of rupture strike lower than this, AB should be increased to about 4 feet, and B and C joined.

Third. An elliptical arch, major axis, or span, 30 ft.; semi-minor axis, or rise, 10 feet; height of abutment, 6.5 ft. (Fig. 6). Using formulæ (1) and (4) we have,

$$e=2 \text{ ft.}$$

$$E=(1.41+0.154\times30)\sqrt{\left(\frac{6.5+0.54\times10}{6.5+10+2+2}\right)\left(\frac{0.84\times30}{0.465\times10+2}\right)}$$

$$=6.03\sqrt{0.58\times3.79}=8.92 \text{ ft., say 9 ft.}$$

For the length of joint of rupture, since $\frac{v}{s}=\frac{1}{3}$, we have, formula (*a*) No. 8,

$$l=1.8\times2=3.6 \text{ feet.}$$

We have now all the necessary data for our work, but before going into the detail of their application, it may be well to

Fig. 6

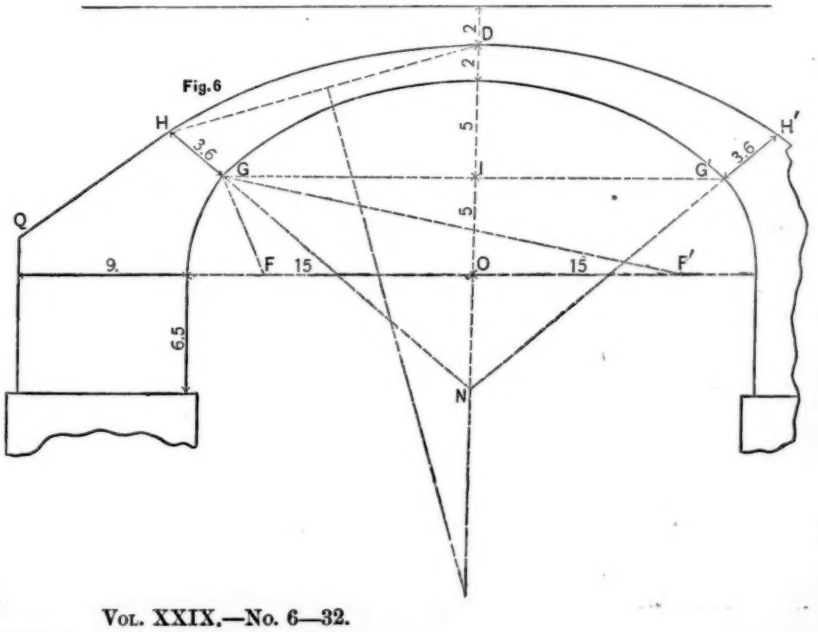
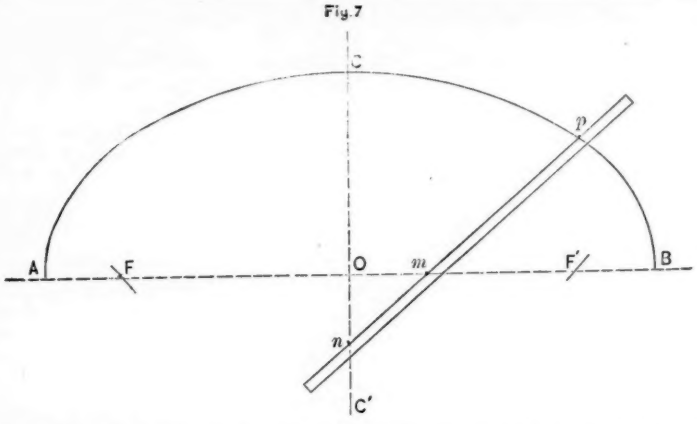


Fig. 7



describe the method given by Monsieur Dubosque for striking the curve, as follows: Draw the horizontal line AB (Fig. 7), and through O erect the perpendicular CC′. Lay off AB=major axis, and OC=semi-minor axis. Then take a rule, or strip of stiff paper with a true edge, and mark upon it a distance np=semi-major axis AO. Mark off, also, pm=semi-minor axis OC. The distance nm is then the difference of the two semi-axes. If now the rule or paper strip be moved in such a way that the point m is constantly upon the major axis, and the point n upon the minor axis, the point p will always indicate a point upon the ellipse. The curve being traced, from C as center with the radius OA, describe an arc cutting the major-axis at F and F′, which will be the foci of the curve.

The position of the joints of rupture GH, G′H′, is found by drawing the horizontal line GG′ at a distance OI= half the rise. It is an essential condition that these joints be drawn normal to the curve of the intrados. We therefore draw the radii rectors FG, F′G, to the point G, and bisect the angle FGF′ by the line GN, on which, produced, we take GH=3.6 feet, the length of the joint of rupture. G′H′ is similarly obtained. We have now the three points HDH′ of the extrados, which we may join by an arc of a circle obtained as for the other arches, as has been done in Fig. 6. An elliptical extrados involves a somewhat complicated drawing, and appears to offer no practical advantage.

The tangents HQ and H′Q′ are then drawn to their intersections Q and Q′, with the vertical backs of the abutments, and our drawing is complete.

Fourth. A false ellipse, struck from three centers; 30 feet span, 10 feet rise; height of abutments, 6.5 feet (Fig. 8).

Arches of which the intrados are false ellipses, called by the French "basket-handled" arches, are struck with an odd number of circular arcs, the centers of those next the haunches being situated upon the horizontal line passing through the two springing lines. It is an essential condition that these several arcs of circles should be tangent to each other at their points of junction. It is the fulfillment of this condition which constitutes the difficulty of correctly drawing the intrados of such arches when, besides, a fixed rise must be adhered to.

I will first describe the method of Monsieur Michal, by which the intrados of the above 3-centered arch may be struck.

Upon AB, as diameter, describe the semicircle AcB, which trisect in m and m'. Join Bm, mc, cm', m'A. Through C draw CM parallel to mc, intersecting Bm in M. CM′ is similarly drawn parallel to cm' intersecting Am' in M′. Draw the radii mO, m'O, and through the points M and M′ draw MO′, M′O′, parallel to mO and m'O, cutting the vertical axis CO, produced, in O′, and the horizontal line AB in D and D′. Then, from D and D′ as centers, with the radius DB=DM=D′A=D′M′, describe the arcs BM, AM′, and from O′ as center

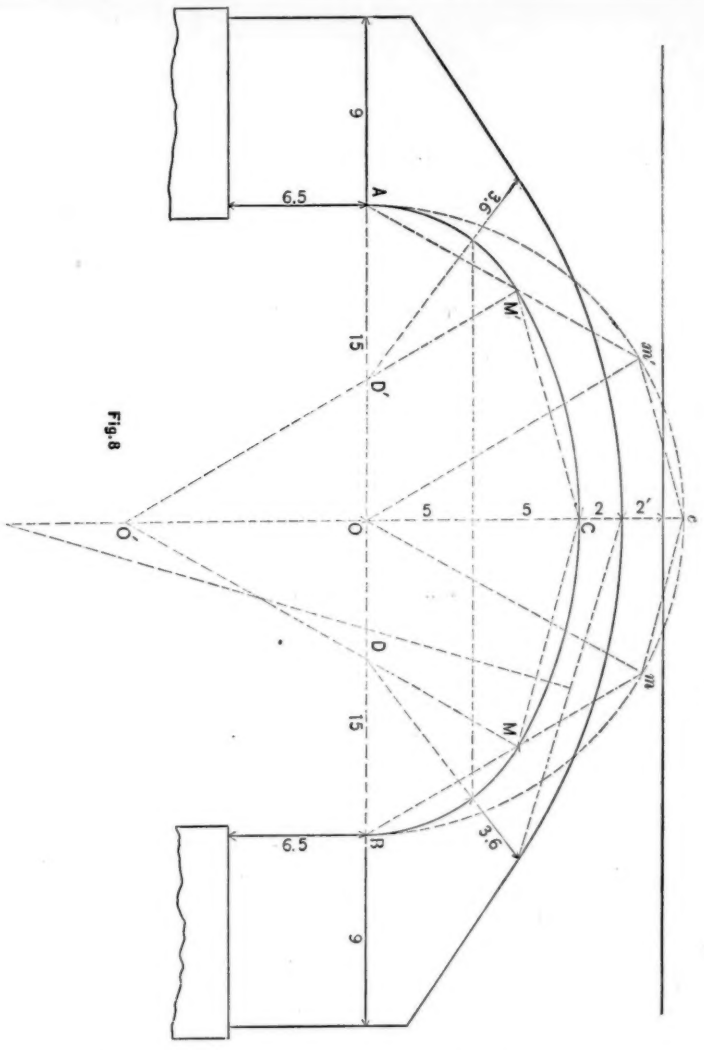


Fig. 8

with the radius O′M=O′M′, describe the arc MCM′.

We will now establish the governing dimensions of the arch, using formulæ (1), (4), and (*a*) No. 8, from which we obtain,

$$e=2 \text{ ft.}$$
$$E=9 \text{ ft.}$$
$$l=3.6 \text{ ft.}$$

The joint of rupture is situated at the distance of half the rise above the springing line.

The extrados is formed by a circular arc, as already described, between the two joints of rupture, and tangents intersecting the vertical backs of the abutments.

A full discussion of basket-handled arches, and the various methods of drawing them, would be a very long affair. I will only remark that a three-centered

arch is applicable only to cases where the ratio $\frac{v}{s}$ is not less than $\frac{1}{3}$. When it falls below $\frac{1}{3}$, more centers should be used.

Monsieur Dubosque gives the following formulæ for calculating the radius of the two smaller arcs, and of the larger central one, as follows:

$$r=a-1.366\ (a-f),$$
$$R=2a-r=a+1.366\ (a-f),$$

in which r=radius of smaller arcs; R=radius of larger arc; a=half span. f=rise. If we make $f=\frac{2a}{3}$, we have,

$$r=0.2725S,$$
$$R=0.7280S,$$

S being the span of the arch. These ratios are not far from $\frac{1}{4}$S and $\frac{3}{4}$S.

THE TRANSMISSION OF ENERGY.

By Prof. Osborne Reynolds, M.A., F.R.S.

From the "Journal of the Society of Arts."

Some few days ago, during a conversation with a friend, I remarked that I was going to give some lectures at the Society of Arts upon the transmission of energy, whereupon my friend inquired—"Is that the transmission of energy by electricity?" To this I replied, "No." The fact is, that I have heard so much about electricity that I began to think it was time to recall attention to the fact that there are other means of performing mechanical operations.

I am not sure whether, during the various lectures which have been given in this room on electricity, the actual term, transmission of energy, has been used. But, whether it has or not, some of the leading ideas connected with it have been before you.

I think it may be said that the great interest which the public has manifested in the recent advance in the arts relating to electricity has arisen, in a large measure, from the cry of joy with which Faure's battery was received. "A cry which said, in so many words, here we have at last a means of utilizing our waterfalls and natural sources of power" in a way that may relieve us of all the anxiety about our coal fields. To those who had studied the subject it was evident at the time that this cry was premature. And to some of us, at all events, it seemed to be a mistake to encourage false hopes, or, rather, knowingly to base hopes on a false foundation, to hold out as a means of replacing our coal what was, in all probability, only another means of increasing its rate of consumption, for every step in art which facilitates the application of power must increase the demand on the acting sources.

But this is not all; the exaggerated claim set up for electricity, diverted for a time, at all events, attention from the true claim, which would have been sufficient in itself had it not thus been put out of sight. It is not our object at present to save our coal, but to turn it to the best advantage to get the greatest result we can, and if Faure's battery or any subsequent advance in this direction conduces to this, it is no small matter. Now, during the last ten or fifteen years, an entirely new aspect has been given to mechanics by the general recognition of the physical entity which we call energy, in different forms.

We recognize the one thing under different forms in the raised hammer, the bent spring, the compressed air, the moving shot, the charged jar, the hot water in the boiler, and the separate existence of coal, corn, or metals, and oxygen. We see in the revolution of the shafts and the travel of belts in our mills, the passage of water, steam, and air along pipes, the conveyance of coal, corn, and metals, and the electric currents, the transmission of this same thing—energy—from one place to another; and in all mechanical actions we perceive but the change of form of the same thing.

Taking this general or energy point of view, we may get rid of all the complication arising from special purpose, and

recognize nothing but the form of energy in its source, the distance it has to be transmitted, and the special form that must be given to it for its application. And this view, although not the best in which to study the special purpose of mechanics or contrivances, is of great importance, inasmuch as it has revealed many general laws, and many fundamental limits to the possibilities of extension in certain directions.

My object in these lectures is to direct your atention to some of the leading mechanical facts and limits revealed by this view.

There is one general remark I would wish to make, by way of caution. I hope nothing I may say will be interpreted by any of my hearers into a prediction as to what may happen in the future. I have to deal with facts, and I shall try to deal with nothing but facts. Many of these facts, or the conclusions to be immediately drawn from them, may appear to bear on the possibilities—or rather, the impossibilities—of art. But in the Society of Arts I need not point out that art knows no limit; where one way is found to be closed, it is the function of art to find another. Science teaches us the re-results that will follow from a known condition of things; but there is always the unknown condition, the future effect of which no science can predict. You must have heard of the statement in 1837 that a steam voyage across the Atlantic was a physical impossibility, which was said to have been made by Dr. Lardner. What Dr. Lardner really stated, according to his own showing, was that such a voyage exceeded the then present limits of steam power. In this he was within the mark, as any one would be if he were to say now that conversation between England and America exceeded the limit of the power of the telephone. But to use such an argument against a proposed enterprise is to ignore the development of art to which such an enterprise may lead.

I wish to do nothing of this kind, and if, in following my subject, I have to point out circumstances which limit the possibillities of present art, and even seek to define the limits thus imposed, it is in the hope of concentrating the efforts of art into what may be possible directions, by pointing out the whereabouts of such barriers as science shows to be impassable.

Although the terms energy and power are in continual, we might almost say familiar, use, such use is seldom in strict accordance with their scientific meaning. In many ways the conception of energy has been rendered popular, but a clear idea of the relation of energy to power is difficult. This arises from the extreme generality of the terms; in any particular case the distinction is easy. I was going to say that it is easiest to express this distinction by an analogy, but, as a matter of fact, everything that seems analogous is really an instance of energy. Power may be considered to be directed energy; and we may liken many forms of energy to an excited mob, while the directed forms are likened to a disciplined army. Energy in the form of heat is in the mob form; while energy in the form of a bent spring, or a raised weight, matter moving in one direction, or of electricity, is in the army form. In the one case we can bring the whole effect to bear in any direction, while in the other case we can only bring a certain portion to bear, depending on its concentration. Out of energy in the mob form we may extract a certain portion, depending on its intensity and surrounding circumstances, and it is only this portion which is avail able for mechanical operations.

Now, energy in what we may call its natural sources has both these forms. All heat is in the mob form, hence all the energy of chemical separation, which can only be developed by combustion, is in the mob form; and this includes the energy stored in the medium of coal. The combustion of 1 lb of coal yields from ten to twelve million foot-pounds of energy in the mob form of heat; under no circumstances existing at present can all this be directed, nor have we a right, as is often done, to call this the power of coal. What the exact possible power is we do not know, but probably about four-fifths of this, that is to say, from eight to ten million foot-pounds of energy per pound of coal is the extreme limit it can yield under the present conditions of temperature at the earth's surface. But before this energy becomes power it must be directed. This direction is at present performed by the steam-engine, which is the best instru-

ment art has yet devised, but the efficiency of which is limited by the fact that before the very intense mob energy of the fire is at all directed, it has to be allowed to pass into the less intense mob energy of hot water or steam. The relative intensities of these energies are something like twenty-five to nine. The very first operation of the steam-engine is to diminish the directable portion of the energy of the pound of coal from nine millions to three millions. In addition to this there are necessary wastes of directable energy, and a considerable expenditure of already directed energy in the necessary mechanical operations. The result is that, as the limit in the very highest class engines the pound of coal yields about one and a-half millions of foot-pounds; in what are called "first-class engines," such as the compound engines on steamboats, the pound of coal yields one million, and in the majority of engines, about five or six hundred thousand foot-pounds. These quantities have been largely increased during the last few years, as far as science can predict, they are open to a further increase. In the steam-engine, art is limited to its three million foot-pounds per pound of coal; but gas engines have already made a new departure, and there seem no reasons why art should stop short of a large portion of the nine millions.

Other important natural sources of mechanical powers are energy in an already directed or army form, wind and water power. Here the power needs no development, but merely transmission and adaptation, and hence it has one important advantage over the energy of chemical separation. But there appears to be what are greater drawbacks—in the irregularity of these forces as regards time, and the distribution as regards space. These have both been, and are, good servants to man.

The application of the power of the wind to the propulsion of ships has, doubtless, influenced the economy of the world more than any other mechanical feat; and, not very long ago, water-power played no relatively unimportant part of the work of the world. But it would seem that both these have had their day, and are now relegated to a work of a secondary kind, not necessarily so. Some further development of art might bring them to a foremost place again, by developing their use to a hitherto unprecedented extent. Hitherto both wind and water have only had a local application—that is to say, they were used where and when they were wanted. Wind was only used in the sailing of ships on voyages, and for mills, distributed so as to be within range of such corn as was too far from water; while water-power, though very valuable to a certain limited extent, when near habitable country, was otherwise allowed to run to waste, and these wastes included by far the larger sources of this power—the larger rivers and water-falls, the tidal estuaries, and last, but not least, the waves of the sea, a source which has never been utilized for good. A modern idea is, that it needs nothing but a possible development of art to render these larger sources not only available for power in their immediate neighborhood, but available to supply-power wherever it is wanted, and so displace the coal, or replace the power as coal becomes exhausted. The desirability of such a result fully explains the entertainment of the pleasant idea; but, unfortunately, when we come to look closer into the question, the probability of its accomplishment diminishes rapidly. Many of the considerations of which I shall have to speak bear directly on this question; so that I shall now defer its further consideration, merely pointing out that, to accomplish this result, the power must not only be extracted from the water on the spot and at the same time, but it must be transmitted over hundreds or thousands of miles, and must be stored till it is wanted.

It may well be thought that energy in a directed form, or in the army form, may be better transmitted than in the undirected or mob form. As a matter of fact, however, energy has never been and never can be transmitted as mechanical power in large quantities over more than trifling distances, say, as a limit, twenty or thirty miles. I say never can, because such transmission depends on the strength of material; and unless there is some other material on the earth of which we know nothing, we know the limit of this. This is a part of my subject into which I shall enter more closely in my second or third lectures.

In depreciating the idea that wind and

water will ever largely supply the place of coal, I do not for a moment wish it to be thought that I take a gloomy view of the mechanical future of the earth. This, I believe admits of immense development, and will not for long depend, as it does at present, on the adjacency of coal fields. This will be explained as I proceed.

It must not be forgotten that, after all, the most important source of energy is not coal, but corn and vegetable matter. The power developed in the labor of animals exceeds the power derived from all other sources, including coal, in the ratio of, probably, 20 or 30 to 1; so that, after all, if we could find the means of employing such power for the purposes for which coal is specially employed—such as driving our ships and working our locomotives—an increase of ten per cent. in the agricultural yield of the earth would supply the place of all the coal burnt in engines. The energy which may be derived from the oxidization of corn has as yet only been artificially developed in the form of heat, and this may be the only possible way; but physiology has not yet advanced to the point of explaining the physical process of the development of energy consequent on the oxidization of the blood; and it is at all events an open question whether the energy of corn may not be really a form of directed energy, in which case corn would yield six or eight times as much energy as coal does at present, consumed in our engines. As consumed in animals it yields a larger proportion of energy—two or three times as much, and may be more—whereas, by burning it in steam-engines, we cannot get half as much. Should we find an artificial means of developing anything like the full directable power of corn—a problem which has not yet been attempted—coal would no longer be necessary for power. I do not mention this as a prediction, but as showing that there are, besides wind and water, other, and as yet untried, directions from which mechanical energy may be derived in the future.

Electricity is not a natural source of energy, for the simple reason that the metals have mostly been burnt or oxidized during the past history of the earth. But still it is important, at this stage of my lecture, to point out that the energy consequent on the separate existence of metals and oxygen can be developed without combustion, in a totally directed form, *i. e.*, totally available for power.

There are many peculiarities which distinguish the group of elementary substances we call metals, but there is no more distinctive feature than this. This is not a primary source of power, but, as it at present appears, it promises to become the most important secondary source. We cannot find metals existing in a separate form, but by the use of power; where and when it exists we can separate them from the salts, and so store the energy in a form completely available for power. The economical questions relating to such storage of energy will be considered in their place later in the course.

It is not, however, only as affecting storage of power that electricity demands our attention, it also affords a means of transmitting power, which has long held an important place in art, and to which all eyes have been recently turned in expectation of something new and startling.

Before considering the developments of art, and the circumstances on which their further development depend, I shall turn, for a moment, to the processes of nature. The mechanics of the universe, no less than those relating to human art, depend on the transmission of energy. In nature, energy is transmitted in all its forms and under all its circumstances, both those which we can imitate in art, and those we cannot.

The most important point with regard to the artificial transmission of energy is the proportion of power spent in effecting the transmission, and the circumstances on which this proportionate loss depends. Is such loss universal? So far as we know, it is attendant in a greater or less degree on all artificial means of transmission, and on all transmissions effected by nature on the surface of the earth. If it were not, this earth would be no place to live upon. No motion would ever cease. As it is, the winds and waters are rapidly brought to rest by the friction which they encounter. Currents of wind and currents of water form the principal means by which energy is transmitted over the surface of the

earth. But there are other means which experience less resistence. Oscillatory waves, those of sound, are a very efficient means of transmitting energy. Sounds are not transmitted to an unlimited distance, chiefly because, by the spreading of the wave the sound becomes weaker and weaker as it proceeds. It is also destroyed by the friction of the solid surface of the earth. Hence the sounds which reach us from bodies high up, as the explosion of a meteor, are heard much further than such sounds made at the surface of the earth, although there are two records of artillery having been heard two hundred miles. Owing to such incidental destruction of sound we cannot say from experience that sound waves in foul air are destroyed, but from the physical properties of gases we know they are.

Waves on the sea are another very efficient means of transmitting power, a means which may be called nature's mill. The waves which take up the energy or power from the wind in mid ocean travel onwards, carrying this energy, and experience such slight resistance that they will, after travelling hundreds or thousands of miles, destroy the shores on which they expend the last of their energy. If we could find a means of utilizing the energy of waves, we should not only save our coal, but also save our country from the waves; still, water waves experience resistance which we can better estimate theoretically than practically.

These are the principal ways in which energy is transmitted from one part of the earth to another. There are others, such as earthquakes, but they all show the same thing, that power is spent in the transmission of energy.

If we look away into interstellar space the case is altered. Here we see two ways in which energy is transmitted—heat, or light, and the motion of the heavenly bodies. In neither of these can we see any direct evidence of resistance or loss of power; and, as judged by any terrestrial measure, there certainly is none. The distance at which we see stars is a sufficient proof of the freedom with which the wave of light travels; while the regularity of the motion of the planetary bodies shows that they encounter no sensible resistance. Yet, although not directly perceivable, there are circumstances that strongly suggest that in both these forms, transmission of energy is resisted. If space is unlimited, and there are stars throughout it, why do not we see them at greater distances than we do? Under these circumstances there could be no spot in the heavens at which at a sufficiently great distance there was not a star, so that, if the light were not stopped, the whole heavens would be one fiery envelope as bright as the sun. This is a question which philosophers have not decided. But one, and the favorite way out of the difficulty, is to suppose that the light does encounter resistance, even in interstellar space. This is a subject on which your Chairman of Council has boldly launched; and whether his hypothesis be right or wrong, it has brought to the front a very interesting subject.

With regard to the resistance encountered by the planetary bodies, our evidence is even slighter. A few domesticated comets seem to diminish their speed; and it is not so long since we were all on the *qui vive*, by the promise of the spectacle of an old friend, who seemed to have come earlier than he was expected, on purpose to verify a prediction of plunging into the sun; but instead of doing so he passed away and was pronounced a stranger, to the joy of the nervous, but somewhat to the discomfiture of astronomers.

The energy which we derive from the sun comes to us in the form of sunshine, in a highly directed but extremely scattered form, being uniformly distributed over all the illuminated disc of the earth. It reaches the outer atmosphere nearly in the same condition as it left the sun, having traversed ninety odd millions of miles without any sensible expenditure of power. In the twenty or thirty miles of the lower atmosphere, however, it encounters very great, but variable, resistance. Sometimes half of it, or three-quarters of it, may reach the earth's surface. This is rare in our country, and on the average not more than a very small fraction ever reaches the surface.

When the sun does shine, the sunshine is a form of energy which may be, and is, very largely directed so as to yield power. Any such direction which may be accomplished by human art is undertaken at an

enormous disadvantage, on acconnt of the scattered manner in which the energy reaches us. The sunshine must be collected before we can make any mechanical use of it.

In the abstract, there are two methods. The one would be to accumulate the energy of sunshine on a given place, over a long time. This is nature's method. The energy on each portion of the earth's surface, during days, weeks, the whole year, or many years, is accumulated by the growth of vegetables. Corresponding to this, however, art has as yet developed no means whatever. If we don't use the sunshine as it falls, energy is lost for all mechanical purposes. I say if we don't, not that we do use it, but because we can, and have done so in a small way. By means of a lens, or reflectors, the sunshine which falls on a certain place may be concentrated on to a smaller space, and so be sufficient to perform some mechanical operation. In this way, small vapor engines have been worked by sunshine. But the cost of tke apparatus necessary for such concentration is out of all proportion to the result accomplished, and shows the art difficulties must be got over by a new departure. There is one further consideration that sunshine on land is too valuable for the maintenance of vital energy to allow of its being devoted to mechanical purposes.

As regards the perfectness of nature's method, so far as I know, no attempts have ever been made to test this. It is probably very wasteful, as are all nature's methods, but it is effective. In the first instance, the energy of sunshine is stored on the spot where it falls, in the tissues, but chiefly in the sap of the grass and vegetation. If this is not removed, a large portion of the energy of the year's growth, that which is in the sap, is stored in the seed, and the rest, although apparently again scattered on the decay of the tissues, is to some extent preserved in the ground, and either forwards the next year's crop, or takes the permanent form of peat; and our coal fields are but evidence of the way in which the directable energy of sunshine has been stored under circumstances where there was no immediate purpose for which to apply it. Under present circumstances, however, this energy is almost everywhere too valuable to admit of secular storage.

It is either moved directly by nature's method, the teeth of animals, or allowed to accumulate for a longer period, and then removed by human industry. The further aggregation of this energy involves the transmission of energy in a mechanical sense, and hence involves the expenditure of power. Nature works by means of directly converting this energy into power. The plant accumulates the energy of sunshine, the animal collects and appropriates this energy. This collection is accomplished by the expenditure of power, which means a redistribution of that portion of the energy which is capable of direction. The scheme of nature, therefore, is a cycle. The vegetation accumulates the energy, so far as time is concerned, leaving it in a scattered form, requiring power to collect it; this power is in the grass, and only wants direction; this it receives in the animal, which again expends some of the energy in the operation of collecting. If vegetable energy be supplied to the animal in a collected form, then a large portion of the directed energy is available for mechanical purposes. And in this way we may form a rough estimate of the directed energy to be obtained from sunshine in this country. The common agricultural rule is one horse or bullock to two acres, such a horse pulling 120 lbs. at a rate of 3.6 feet per second for eight hours a day. That is a nominal horse.

We thus get something like 3,000,000,000 over and above the energy necessary for the energy spent in eating the corn and moving itself, which we must put down as at least equal in amount. Taking only the available portion, we have the equivalent per acre of nearly three tons of coal burnt in such steam-engines as exist at present. Now the average weight of the vegetable produce from one acre, taking the form of straw and corn, would be about two tons. So that, as far as mechanical power is concerned, coal burnt in our present steam-engines, and corn and straw eaten by horses, yield about the same energy, weight for weight.

The energy which we derive from sunshine is scattered all over the earth, and if it is to be utilized at any spot other than that at which the sunshine falls, it must be transmitted by the expenditure of power.

The energy required for immediate operations of agriculture absorbs a large proportion of the actual energy grown. The surplus is available for purposes of art, and we may say that the primary object of man has been to render this surplus as large as possible. This is accomplished, in the first instance, by applying the residue of energy to so ameliorate the conditions of agriculture as to increase the yield and diminish the labor. In this way the land is leveled, enclosed, and drained; buildings are erected, and finally, but most important of all, roads are made. The effect of roads in increasing the surplus energy is probably greater than any other human accomplishment. The only means of transmitting for purposes of collection or other purpose aggregate energy in the shape of corn, without roads, is on the backs of animals. In this way two or three hundred miles was the absolute limit to the distance an animal could proceed, carrying its own food. On a good road, a horse will draw a ton of food at twenty miles a day, which would mean that it would proceed 800 miles before it had exhausted its supply, or whatever surplus energy there might be available on one spot, half this would be available at 400 miles distance. The labor of maintaining the roads should, of course, be deducted, but this is very small.

The labor of constructing canals is very great, but the result is equal; a horse can move 800 tons twenty miles a day; or a horse could draw its own food for 80,000 miles on a canal. That is to say, with a canal properly formed, a horse could go five times round the world without consuming more energy than was in the boat behind it. Or corn could be sent round the world with a consumption of one-fifth. On railways, at low speeds, the force required is about ten times greater than on a canal, so that the expenditure in going round the world would be about equal to the total energy drawn. If for a moment we replace the horse by the steam-engine, and the corn by coal, we have to add the weight of the engine to the coal, and diminish the efficiency by one-third; we so get that the consumption of coal for the same load of coal as of corn, would be about double, or an engine would go about one-fourth round the world, consuming in coal the net weight in the train, that is exclusive of carriages and engine. Or for every thousand miles corn is carried by rail, something like 10 per cent. of the energy of the corn is expended in draft. This is exclusive of the expenditure in wear and repairs, which will be certainly equal, if not greater. Taking, then, the mean distance by rail between London and the West of America, as 2,000 miles, the present expenditure in the energy of corn in transit is somewhere about 10 per cent. The expenditure of energy on the ocean varies, but if transported by steam it would be probably 10 per cent. more, so that at the present time we are actually receiving available mechanical energy, transported in the form of corn, over 2,000 miles of land and 3,000 miles of sea, entirely by artifically directed power, with an expenditure of less than 50 per cent.; a proportion which 200 years ago would have had to have been spent in transmitting it fifty miles over land; a result which has been accomplished by the employment in the mean time of the residual energy over and above that necessary for agriculture, together with a further supply drawn from our coal beds.

Turning now our consideration to coal, we find per weight as used at present, this yields rather less power than corn, but not less than two-thirds, and it then appears that coal may be transmitted at the present time, between any two places on the earth which are connected by rail and water, with an expenditure of less than 50 per cent.

In instituting this comparison, the standard has been the actual available power, as developed in our present engines and in horses, with which, weight for weight, there is not much difference. But the adaptability of this energy, so developed for particular purposes, renders the one medium much more valuable than the other. Thus for agricultural purposes, weight for weight, horse food is worth in money ten times as much as coal. This shows the extreme difference in the value of energy according to its adaptability; and the extension, for which there is unlimited scope of the adaptability of steam power, may render it ten times as valuable as at present; nor would this be any small proportion compared with the total energy employed in

the work of the world. In this country there are said to be between two and three million horses, and we may put the laboring men down at five millions, or the total power derived from corn down as over three million horses. From the best information going, the work done by steam in this country does not exceed the labor of two million horses, so that more than half the energy is still derived from corn. A greater proportion of the actual corn used for horse food comes across the Atlantic; and for many years maize was sold in this country at an average price of £6 or £7 a ton, the cost of transit being a very small matter. Of course the same cost, say £1 per ton, applied to coal would be a serious matter, considering the low price of the latter. But if, in the present state of our art, energy can be transmitted by corn from any part of the world to this country with an insensible rise, there is no reason to suppose but that, with the advance which science shows us, there is every reason to expect coal may be transmitted with a corresponding small increase in its cost, wherever the demand for it is sufficient to recompense the outlay necessary for opening the roads or canals.

We come now to consider other means of transmitting energy in smaller quantities applicable to its distribution for immediate application. Such transmission is not a matter of secondary importance, although the distances over which it is transmitted may be comparatively insignificant. To emphasize this, I may recall what was previously mentioned, namely, that the relative price of corn and coal shows that the power given out by horses is at least ten times as valuable as that of steam, for more than half the purposes for which energy is used; or that it answers better to burn our coal in bringing corn from America to plough in England, than to use the coal here for ploughing.

In fact, for most of the detailed purposes for which power is used, to draw it from a large source (such as a steam-engine), distribute it and adapt it to its purpose, is ten or twenty times more costly than its transportation in large quantities over thousands of miles.

Now the means of artifically transmitting power may be considered as three. The power may be stored in matter in various ways, and the matter with the energy transported—as, for instance, in our watch springs. The second means is the transmission of power by moving matter, without actually storing the power in the matter—as in shafts and belts, hydraulic connection, &c. And the third method, which is distinct from the others, is the transmission of energy, in the form of heat or electricity, by the flow of currents through conductors; in this way all the power in the steam passes through the boiler-plates from the furnace into the boiler. Of course, each of these means includes an infinite variety of detailed contrivances, more or less dissimilar. But there is good reason for classing them under these three heads, for all the contrivances under each of these heads are subject to the same general limits, whether those of efficiency or distance.

There is one thing in common to all these means of transmission, and that is that they all involve a material medium. The quantity of matter required constitutes a primary consideration in all of them. This quantity of matter is fixed by what we may call the properties of matter, one of the most important of which, as regards the first two means, is the possible strength of material. Looking round. we see the effect of the limited strength of material in all nature's works. Of course it may be that we shall be able to work stronger materials than we have at present. Organic materials, such as the feathers and tissues of animals, are stronger than steel, weight for weight, so that there is a possibility of improvement, but that man will go beyond nature in constructing organic fibre seems improbable, and such possibility of improvement as exists may be discounted. At present we may set down our strongest working material as steel, the art of working in which is so perfect, that we may calculate on nearly the greatest strength for all purposes. I have taken fifteen tons on the square inch as the limit of safe working tension, in making the estimates which I shall now bring before you. First of all, I will ask your attention to the possibilities of transporting power in a stored form.

The question of economy in the conveyance of energy in a stored form is simply one of the intensity with which it

can be stored. If we want to carry energy about, we must have it stored in some material form—and this material has to be carried by ordinary means—so that the question of economy is simply the amount of available energy that we can store in a given amount of material.

If energy, stored in a particular manner, is more readily available for some special purpose than that stored in another, then it may, on the whole, be more economical to carry it in that form. This is abundantly illustrated in our watch springs.

The greatest amount of energy that can be stored in a given weight of steel is very small, compared with other means. To take a familiar unit, to store the energy necessary to maintain one horse-power for one hour would require no less than fifty tons of steel—that is to say, fifty tons of steel in the form of watch springs, all fresh wound-up, would not supply one horse-power for one hour; and yet this is the commonest form in which energy is carried about.

It is the adaptability of the spring, and the readiness with which energy can be put in and taken out, which recommend the steel spring.

India-rubber will store much more energy than the same weight of any other material, say, eight or ten times as much as steel; but with this, several tons would be required to store the horse-power for one hour. A much more capacious reservoir, according to its weight, is compressed air. There are certain difficulties in getting the energy in and out without loss; but with air, compressed to four times the pressure of the atmosphere, we should only require about 20 lbs. of air to yield the amount of one horse-power for one hour. Of course, if we were going to carry this air about, to the weight of the air would have to be added the weight of a case to contain it, and such a case, in the form of steel tubes, would weigh something like 230 lbs.; so that, in any form in which we can carry compressed air about, we shall have about 300 lbs. to carry for each horse-power per hour.

Another means of storing energy, very largely used, is hot water. This is largely used in a way not always recognized. The boiler serves another purpose besides that of converting the energy of the furnace into the power of the steam. It stores the power, and equalizes the stream between the fire and the engine, a function the importance of which has been brought to the front in the recent efforts to apply electricity for communication of power, where the want of a similar reservoir between the generator and the motor has, in many cases, proved fatal to the enterprise, a want which secondary batteries are now being used to meet. Hot water has also been employed as an independent reservoir, and as such it is better in some respects than compressed air. The fundamental limits are of much the same kind. In this case, however, the absolute limit is temperature. The vessel in which the water is carried must be strong enough to withstand the pressure, and all materials lose their strength as they get hot. The considerations are here much the same as in the steam-engine, and 400° Fahr. appears to be about the limit. At this temperature, for every 4 lbs. of water the cases would weigh 1 lb., and there would be no advantage of large over small cases; except as a matter of construction, the proportionate weight would be the same. The gross power of a pound of water, the steam being used without condensation, is about 20,000 foot-pounds, or we should require 50 lbs. to store 1,000,000; this is the extreme limit again. The present accomplishment would be about 150 lbs. per 1,000,000 foot-pounds stored—rather less than compressed air. The only other means of packing power, that is at present looked to, is that of the much talked about secondary battery. Here there is a great deal of doubt as to what is actually accomplished; take the most reliable statements, from which it seems that in order to get 1,000,000, something like 100 lbs. of battery is required, which will make this means of storing energy very much the same as compressed air or hot water.

It is important to notice that the initial cost of the energy stored by these means differs considerably. This cost is rather difficult to estimate; but a practical estimate may be formed in this way:

Taking the power, as delivered by the steam-engine, as 1, how much of this power will be given out after secondary storage? Here the hot water has an ad-

vantage, for it is heated directly by the coal, and is all on its way to the steam-engine.

With compressed air, there are three operations, each as costly as the steam-engine, and at least half the initial power is spent during the compression, storage, and expansion; so that the energy is at least double as costly in coal, and six times as costly in machinery. I have put it down as three times as costly as the energy in hot water, but this is considerably below the mark. The electricity has also to go through three operations, and cannot be less costly than compressed air.

Now, if we revert for one moment to the consideration of the main transmission of power, we see at what an immense disadvantage any form of packed energy is, compared with coal or corn; as at present packed it weighs at least 100 times as much.

While the limits imposed by the strength of material render it certain, as far as compressed air and hot water are concerned, that the weight can never be reduced by more than half, these limits are sufficient to show that packed energy cannot be transported over long distances, even if it can be obtained directly from such falls as Niagara. But this is no argument against the importance of these means for short distances and special purposes. As I have already pointed out, our watches show that circumstances may render the very heaviest means the best for particular purposes. And if in any of its forms packed energy were directly available for household purposes, though it cost ten or twenty times as much as power direct from the steam-engine, its use would still be assured..

One fact should be noticed, that in all these forms the power is packed, and needs nothing but drawing off, whereas corn or coal do not contain the power. The oxygen is an equally essential ingredient. In this fact lies the great advantage of corn and coal for transportation. They are really, so to speak, but cheques for power, which can be cashed at any spot where a bank, in the form of a steam-engine or a horse, exists. But, of course, not being energy, they are not generally current—in fact they are worthless, except where the bank exists, and where they represent such small amounts that the banks refuse them. Now these forms of packed power are, so to speak, generally current, that is to say, they are available under almost all circumstances, and in greater or less degrees of smallness; from the very smallest, which is the watch spring in our pockets, which supplies a continuous stream of power in less than one ten thousand millionth of a horse-power; or the Whitehead torpedo, which carries some million foot-pounds of energy under the sea. Perhaps the most pressing purpose for which these forms of packed energy are wanting is that of locomotion.

The distance which a locomotive body, be it animal or machine, can travel, loaded or free, is limited by the ratio of the power which it carries to its gross weight. The speed which it can attain is limited by the rate at which it can use its energy compared with its weight. Hence there are two particulars in which we can compare the different form of stored energy for locomotive purposes.

Let us take the horse and the locomotive. A full-sized horse weighs, say, 1,500 lbs., and, at a rate of 2½ miles an hour, will go five hours without food, doing about 10,000,000 foot-pounds of work, including the work necessary to move itself; this represents the largest result, or about 150 lbs. per 1,000,000 foot-pounds. If the horse is put to ten miles an hour, it will not do more than 1.5 million foot-pounds in a single journey, besides moving itself. Probably the greatest rate at which a horse can use its energy is about 4,000,000 foot-pounds per hour, or 750 lbs. per horse-power.

A locomotive with its tender, say, weighing 60 tons, exerts 500 horse-power gross—270 lbs. per horse-power per hour; so that a first-class locomotive with tender is above one-fifth as heavy for its power as the horse; but then the horse cannot go more than ten miles an hour.

Now, in a general way, passenger locomotives carry coal and water for eighty or one hundred miles, *i.e.*, two hours; or the locomotive already mentioned expends at one run about 2,000,000,000 foot-pounds; which means that the gross weight of the locomotive is about 60 lbs. or 70 lbs. per 1,000,000 foot-pounds of power with which the locomotive starts.

In thus taking the gross weight of the

horse or locomotive. we must remember that this includes the weight of carriage and machinery, and that in whatever form the energy is carried, this weight must be added. In the locomotive the weight of water and coal in the tender for two hours' journey weighs about one quarter the gross load; and if we add the weight of the boiler, we may consider the carriage and machinery at one-half to one-third the gross load. Taking the latter, and substituting for the boiler, coal, and water; energy in either of the above forms, the coal, water, and boiler would be about 40 lbs. per 1,000,000: so that, if we took compressed air instead, we should have one-fourth the power; or the engine would run for thirty minutes instead of two hours, a distance of twenty-five instead of a hundred miles. A fireless locomotive might do more than this, say, thirty-five minutes, or thirty miles, at the same speed as the locomotive. Faure's battery, if it could be made to work at all, would carry the locomotive forty-eight minutes, or thirty-five to forty miles.

These figures seem to show that the locomotive has little to fear from any of these rivals, that is under circumstances where the smoke and steam are no harm, and where a full-sized locomotive is required. But there are already some cases where the locomotive is required and where the burning of coal is impossible. Should the Channel Tunnel be made, there will be a great field for some form of packed energy; but it does not seem that the promoters have much faith in a smokeless means of working the tunnel being forthcoming. As regards horses, however, there is nothing to show why the horse should not be rivalled by some one of the forms of packed energy. There have been inventors who have constructed carriages to go by clockwork. This has now become possible, substituting hot water, compressed air, or a battery for the spring, and such means have already rivalled the horse on tramways. The fact that horses are not at all used for tramcars is a matter of as much surprise as that steam should be used on underground railways. For locomotives driven by compressed air might certainly be made cheaper and better in every way.

At the present time it would probably answer well, in a pecuniary point of view, to supply in compressed air energy at the rate of 2d. or 3d. per million foot-pounds, provided a sufficient quantity could be required; so that if simple and efficient means of applying such energy to perform the heavier part of manual labor, we might get as much power for 6d. as what a man will do in a day at 2s. But it is the means of applying it that is wanting.

Even for horse work—except where there is a railway or tramway—the mechanical means are wanting. We have no mechanical substitute for the horse's foot. So that although there are more than a million horses in this country continually engaged in the operations of husbandry, where they work in groups so as to get three or four horse-power at one operation, an amount of power not too small for the direct application of steam power; and although for twenty-five years steam-engine makers have been doing their very best to adapt the power of the steam-engine to this labor, which exceeds any other actual application of power, the possibility of steam ploughing with economy is still a question. The use of steam on paved or on macadam roads is much the same, so that, until steam has been applied to such purposes, there is little hope for other forms of stored energy.

Coming back for a moment to Faure's battery, I would carefully point out that the result which I have put down—100 lbs. per 1,000,000 foot-pounds of energy—refers to what has been accomplished, and not to any possible limit. The principles involved in the chemical action of these batteries, in fact in all batteries, are well understood; and so far as these principles are involved, it is easy to define limits; but there are a number of secondary actions which are not so well understood, and which have hitherto prevented any approach to the theoretical limits. In the Faure's battery, the theoretical limits are about 3 lbs. per 1,000,000 foot-pounds. That is to say, the oxidization of 1 lb. of lead to litharge, and the deoxidization of 1 lb. of peroxide, together, yield 360,000 foot-pounds. How far, at present, Faure's battery is within this limit, at once appears something like twenty-four times. Should this be accomplished, power could be

packed at the rate of 1,000,000 foot-pounds for 3 lbs., or say 6 lbs. weight, to allow for wastes, a result which would most certainly displace steam in the locomotive, but which would still leave coal and corn six times the lightest vehicle of power.

It should be noticed, however, that although the means of doing so are still entirely wanting, could other metals, such as iron or zinc, be used instead of lead, the results would be much greater. This is shown by the relative amount of power necessary to oxidize or deoxidize these materials, which we see for iron and zinc are five or six times greater than for lead; here is an apparent opportunity for art.

Should this be realized, then, indeed, coal might be displaced as the cheapest medium for the transmission of power, but that would be a small matter compared with the change that would occur in our ways of applying power. For the dream of Jules Verne, of 20,000 miles under the sea, would become a reality, and, instead of steamboats, we should travel in submarine monsters as yet unnamed, which we may call steam-fish.

But if science as yet imposes no limits beyond those I have mentioned, at the same time it holds out no prospect. The chemistry of these batteries has been very deeply considered, and those who have studied the subject most deeply apparently see no direction in which to direct their efforts; so that any great advance in this art must entail a great discovery in science.

There now only remains for me to consider the transmission of power, as power, or by electricity, a most important branch of my subject.

So far I have spoken only of the conveyance of power by means of coal, corn, or in one or other of the several forms of packed energy. To-night I come to consider the transmission of power by what are more distinctly mechanical methods, and by currents along pipes and conductors. These are the means by which power is almost always distributed, *i. e.*, transmitted from the acting agent, be it horse, water-wheel or steam-engine, to its operation, whatever it may be. In most cases the distance of such transmission is so short as to be the subject of small consideration in determining the means to be employed. That is to say, the means are chosen rather by their adaptability to receive and render up the power than by the efficiency with which they transmit it. Thus, if we take an ordinary mill, the shaft which receives the power from the engine is generally driven at that speed which is best adapted to receive the power from the engine, and deliver it to the machinery in the mill, without considering whether a much smaller shaft might be used if it were caused to run at a much higher speed. Thus, in a mill driven by an engine of two or three hundred horse-power, the shaft which receives the power will generally be five or six inches in diameter, whereas it would be possible to use a shaft of two inches diameter if the efficiency of the shaft were the only consideration. Or, again, take a screw steamboat. The distance from the engines to the screw may be 250 feet, the power 10,000 horse. This could be transmitted by a shaft twelve inches in diameter, if allowed sufficient speed, but the screw has to make sixty revolutions per minute, and this determines the speed at which the shaft is made to run, and hence the shaft is made thirty inches instead of twelve inches. This is because, owing to the smallness of the distance, the efficiency of the means of transmitting the power is a small consideration. There are, however, many circumstances under which it is impossible to bring the source of power close to its work, and then either mechanical power is not used, or the efficiency of the means becomes a consideration.

In other cases it is a question whether it is better to distribute the sources of power, such as steam-engines, so that they may be near their work, or to use one large source, and distribute the power by some mechanical means. This rivalry exists in almost all engineering work which covers a large area, and generally a compromise is come to, engines being distributed about the works, and the power of these distributed to the machines by means of shafting. In many cases separate engines are used for each machine, but not often separate boilers, the power being distributed by steam-pipes.

Dockyards have long afforded a field for the competition of the various means of distributing power. Here, generally,

the distances between the operating machines, such as cranes and capstans, is considerable, and the work required from each machine very casual. And every means of distribution is, or has been, in use, from a separate engine and boiler to each machine, as at Glasgow (separate engines drawing their steam from central boilers), to a complete system of hydraulic transmission, from a central pumping station, as at Grimsby or Birkenhead.

But the question between centralization or distribution of steam-engines is not by any means the only one, or most important one, which depends on mechanical means of distributing power. Every improvement in the means of distributing power from a central engine opens a fresh field for its use.

The considerations relating to this subject are numerous. Hitherto, as regards the main transmission of power, the principal consideration has been the percentage of loss according to the distance, but, as regards the final distribution of power, the form in which it is distributed must be such as admits of its being at once available for its purpose. Thus, hydraulic distribution is favored in dockyards, because it is required for heavy forces and slow motions, but where rapid motion is required, hydraulic distribution gives place to some other.

Again, where the quantity of power that has to be distributed is almost important consideration, the distribution by means of water or compressed air will generally be the most efficient, whereas these would be by far the most costly means for small quantities. It thus has to be remembered that, besides the general question of efficiency, each means has particular recommendations for particular purposes.

It is not, however, with these particular recommendations that I am concerned. My object is to show the limits within which the use of each means is confined, however fit it may be for its purpose. Taking first the mechanical means, which are shafts and ropes, we find that the possible limits to both these means are absolutely defined by the strength of material. The amount of power any piece of material will transmit by motion against resistance is simply the mean product of the stress or force acting in the direction of motion on the section multiplied by the velocity, so that, if the stress is uniform over the section, the work is the product of the area and intensity of stress and the velocity.

In a revolving shaft, neither the stress nor the velocity are uniform over the section, both varying uniformly from nothing in the middle to their greatest value on the outside; so that their mean product is exactly half the product of the greatest values. The greatest power per square unit of section a shaft can transmit is half the product of the greatest stress into the velocity at the outside of the shaft.

Taking, then, the greatest safe working stress for steel at 15,000 lbs. on the square inch; taking what is the greatest practical velocity at the surface, 10 feet per second (the speed of railway journals), the work transmitted is 75,000 foot-pounds per second per square inch of section—135 horse-power; so that we should have to have a shaft of upwards of 7 square inches in section to transmit 1,000 horse-power, that is, a shaft of over 3 inches diameter. The friction between such a shaft and lubricated bearings is well known, .04; so that calculating the weight of the shaft 24 lbs. per foot, we have power spent in friction about 52,000 foot-pounds per mile, that is, one-tenth the total power the shaft will transmit. That is, if we put 1,000 horse-power into a 3-inch shaft, making 500 revolutions per minute, we ought, at the end of a mile, to be able to take 900 horse-power out of it. If we had to go farther the size of the shaft might be diminished, so that in the next mile we should again lose a tenth, and if we repeat this process seven times we shall, at the end of seven miles, have left about half the original power put in.

It will be thought, perhaps, that a 3-inch shaft is very small to transmit so large a force; this is because the speed of 500 revolutions per minute is inconveniently high for purposes of employing the power; but if it were merely a question of transmission it would be about the best speed. This, then, shows the limit of the capacity of shafts as transmitters of work.

Turning now to steel ropes, these have a great advantage over shafts, for the stress on the section will be uniform, the velocity will be uniform,

and may be at least ten to fifteen times as great as with shafts, say 100 feet per second; the rope is carried on friction pulleys, which may be at distances of five or six hundred feet, so that the coefficient of friction will not be more than .015, instead of .04. Taking all this into account, and turning to actual results, the work transmitted per inch would be 1,500,000 foot-pounds per second; or that a $\frac{3}{4}$ inch rope is all that is necessary to transmit 1,000 horse-power in one direction, this would make the loss per mile only $\frac{1}{60}$. But in practice, rope has to be worked backwards and forwards, and the tension in the backward portion of the rope must be half the tension in the forward portion. This reduces the performance from $\frac{1}{60}$ to $\frac{1}{20}$, which would cause half the work to be lost in ten miles. If we use a bigger rope, and run at lower speed, then the coefficient of friction would be reduced to 0.1 and the distance extended to fifteen miles. Experience with ropes is large, and they have been found, without question, to have been the most efficient mechanical means of transmitting power to long distances, but their use is subject to drawbacks. The ropes wear somewhat rapidly, as do also the pulleys on which they run, and this circumstance is very much against their use in any permanent work. Nevertheless, they are used for working mines, steep inclines, and steam ploughs; while at Schaffhausen they have been used for transmitting power to considerable distances.

Turning to the transmission of power along pipes, we find the conditions somewhat modified. The formula for the amount of power transmitted by water is the same, namely, the product of the area of section into the velocity. But the resistance obeys different laws. In the case of shafts and ropes we have seen that the distance is subject to an absolute limit.

In the case of fluid in pipes this is not so, no matter how long a pipe may be, if there is no leakage, water would flow along the pipe until the level of its surface were the same at both ends. But the rate of flow would diminish with the length and diameter of the pipe. Thus, we can transmit power through a perfectly tight pipe, however small, and however long; but when we come to consider the gross power that can be transmitted through a given pipe, with a given per-centage of loss, the question is different. Given the size and strength of the pipe, the gross amount of power and the per-centage of loss, and the limits are fixed. Thus, taking a 12-inch pipe capable of standing 1,400 pounds on the square inch, the loss in transmitting 1,000 horse-power would be about 5 per cent. per mile, at first increasing—as the pressure fell to 700 lbs.—to ten per cent. We should thus have lost half the power in about seven miles. We cannot say that seven miles is the absolute limit, for with a 24-inch pipe, which would cost four times as much per mile, we could transmit the same power thirty times as far with the same loss. The cost of laying a 12-inch pipe for seven miles, however, would probably be as much as even 1,000 horse-power would stand, while a 24-inch pipe for 200 miles would be out of all proportion. Then there is the consideration of leakage, which although very small for short lengths, is larger for greater lengths.

Seven miles is at present an outside economical limit of hydraulic transmission, even for such a large amount of power, but with air the case is different. This flows so much easier than water, that the cost of transmitting the same power through the same distance, with the same loss, would be about 12 per cent., or, at the same cost per mile, the air may be transmitted 100 times as far with the same loss. The total cost, however, would thus be 100 times as great, which would exceed the economical limit; but not only theory but practice has shown that power may be economically transmitted five times as far by air as by water—something like thirty miles. But on comparing these two means, one circumstance must not be lost sight of, and that is, that getting the power into the pipe in the form of compressed air will cost twice as much as getting it in in the the form of water. This is a great advantage for water where the distance is short, but where the distance is long, the greater efficiency of air more than compensates for this initial loss.

Like water, air can only be transmitted economically where the quantity is large, the friction being proportionately greater

in small pipes than in large, varying as the four-fifths power of the diameter.

This is a great drawback, both as regards hydraulic and compressed air transmission. It does not affect ropes and shafts in the same way, but even in these cases considerations of durability prevent these means being used efficiently for the transmission of small quantities of power to considerable distances, so that, with the possibility already mentioned, there remains an opening for any means that will enable power to be transmitted efficiently in small quantities, and such a means we have in the flow of electricity along wires or conductors. In considering electricity we may well start with the questions—1. Will electricity enable us to transmit power in large quantities more efficiently than the foregoing means? 2. Will it enable us to transmit small quantities? These questions may be more definitely answered than they could a few weeks ago. Thanks to the experiments of M. Deprez, who appears to have been the only one, out of all those who are advocating the use of electricity, who has had the courage to try and see what can be done, we can now say with certainty that a current of electricity, equivalent to 5 horse-power, may be sent along a telegraph wire $\frac{1}{8}$ of an inch in diameter some ten miles long (there and back), with an expenditure of 29 per cent. of the power, because this has already been done. In order to do this it would seem that M. Deprez has perfected his apparatus so as to have nearly reached the possible limit. Compared with wire rope this means falls short in actual efficiency, as Messrs. Hems sends 500 horse-power along a $\frac{3}{4}$-inch rope. To carry this amount, as in the experiment of Deprez, one hundred telegraph wires would be required; these wound into a rope would make it more than 1.4 inches in diameter, four times the weight of Mr. Hems' rope. With the moving rope the loss per mile is only 1.4 per cent., while with the electricity it was nearly 6; so that, as regards weight of conductor and efficiency, the electric transmisssion is far inferior to the flying rope. Nor is this all. With the flying belt Mr. Hems found the loss at the ends, in getting the power into and out of the rope, $2\frac{1}{2}$ per cent.; whereas, in M. Deprez's experiment, 30 per cent. was lost in the electric machinery alone, which is very small as such machinery goes. But this is not all. No account is here taken of the loss of power in the transmission to and from the electric machinery, a matter which is, I believe, very much under-estimated.

The machines made revolutions at 1,000 and 700, much too high for direct connection with either a steam-engine or any mechanical operator; the power, then, had at each end to be transmitted through gearing, or a system of belts. And supposing this alteration of speed to have been five or six at each end, experience tells us that a loss of at least 15 per cent. must ensue. This loss was indeed apparent, for the dynamo-meter was connected with the machine with a belt, which showed a loss from this one belt alone of 20 per cent. Taking the whole result, it does not appear that more than 15 or 20 per cent. of the work done by the steam engine could have been applied to any mechanical operation at the other end of the line, as against 90 per cent. which might have been realized with wire rope transmission. To set off against this electricity has the enormous advantage in the conductor being fixed, and in the fact that it is likely to be, if anything, less costly and more efficient for small quantities of power than for large. These advantages will certainly insure a very large use for electricity in the distribution of power, particularly for high-speed machinery.

There is yet another means of communicating and distributing energy now coming rapidly into vogue. This is by the transmission of coal gas along pipes. The distances, often many miles, through which the gas is often transmitted before reaching the engine, are such that, with any other means of distributing power, would considerably enhance the cost of the power. But in the case of gas it does not appear that these distances are at all a matter of consideration. This may be at once explained. It takes about ten cubic feet of gas to develop 1,000,000 foot-pounds in a gas engine, whereas of air compressed in the ordinary way it would require something like 140 cubic feet to yield the same power. Hence, the comparative cost of transmission is the cost of transmitting ten cubic feet of gas against that of 140 cubic feet of compressed air, and these would be about as

one to twenty-five; so, as a means of distributing energy, gas is twenty-five times more efficient than compressed air.

I have now placed before you, as far as circumstances will allow, the various means by which energy, in a form available for power may be transmitted over long distances, together with the circumstances which limit such transmission. By means of the railway and steamboat corn and coal can be, nay is, transmitted half way around the earth with an expenditure of power of less than half the power represented by the coal carried, but this can only be done where the quantity to be transmitted is very large.

At present the efficiency is unrivalled, no means of packed energy or of current energy approaching even 1 per cent. And, further, there is apparent room for a large diminution in the present expenditure, small as it is in the improvement on the steam-engine as a means of directing the energy of coal. For the distribution of power, this means ceases to be efficient, nor can it be employed to transmit energy which has already taken the form of power. For these purposes other means have to be employed. These various means, although they differ greatly in efficiency, all fall so far below the efficiency of coal and corn, that a hundred miles appears to be the outside limit any economical transmission of power, in quantity for mechanical purposes, could be at present effected; and hence any power, be it derived from wind or water, must be used within this radius of its source; and, except in places far out of the reach of rail or water, this limit may be divided by ten.

So far as efficiency of transmission in considerable quantities, neither secondary batteries nor electrical transmission are more efficient than compressed air or belts, but when it comes to transmitting small quantities then electric transmission has a decided advantage. The cost of the electric conductor diminishes with the quantity to be transmitted, and by making the conductor sufficiently large, its efficiency may be increased to any extent.

At the present time, electric conductors are continuous half way around the world, and whenever a message is sent from England to Australia direct energy is transmitted 10,000 miles, but in what quantity? The energy of the current, as it arrives, is not much more than sufficient to keep a watch going, at any rate not more than $\frac{1}{1000}$ millionths of horse-power. The value of such energy, estimated at £17 per minute, would be equivalent to a billion pounds per horse-power per hour, whereas the highest price paid for animal labor in Australia or England is not more than 6d. per horse-power per hour. This shows the difference between the transmission of electricity for telegraphic purposes and its transmission for mechanical purposes. Energy differs in value greatly, but for operations that can be performed by men or horses, the price of energy must be regulated by the highest price of corn.

The prosperity of any spot in the past depended on the fertility of the adjacent soil. But the use of coal has altered this, and now the present prosperity of this country is owing to the adjacency of our coal fields, these having rendered it possible to bring our food across the earth. The improved means of transmitting coal and corn, it would seem, have, or may again, change this; and if, instead of looking on the life of this country as limited by the life of our coal fields, we look boldly forward and foster every means, political, social, and mechanical, which may render this a favorite spot to live upon, we need not fear that the necessity of bringing our coal from a distance will make a difference which will counterbalance the advantage we shall derive from the mechanical facilities we shall have here.

In a recent *Chronique Industrielle* an account is given of the examination of a large cylinder in a Woolf engine employed in the mines of Sarrebruck. On opening the cylinder there was found upon the piston a brown, wax-like mass, weighing more than 150 kilogrammes. It contained 60 per cent. oxide of iron, 26.77 per cent. of organic matters soluble in alcohol, 5.7 per cent. of insoluble organic matter. The residue being composed of water with a little silicic acid. The cylinder had been in use for about a year, during which time 192 kilogrammes of suet had been employed for lubrication. The decomposition of the suet by steam into glycerin and fatty acids led to the formation of a soap of protoxide of iron. The oxidation of the iron, which is limited chiefly to the interior surface of the cylinder, gradually produces an enlargement of the diameter. The evil may be obviated by using as a lubricant mineral oil of good quality, which boils only at a very high temperature.

THE DRYING OF GUNPOWDER MAGAZINES.

By PROF. CHARLES E. MUNROE, U. S. N. A.

Proceedings of Naval Institute.

In the Ordnance Instructions of the United States Navy, paragraph 1233, page 341, it is directed that, in order to absorb the moisture from a magazine, chloride of *lime* or charcoal should be suspended in an open box under the arch, and that it should be renewed from time to time.

On reading this I felt assured that an error had been committed, and that it had probably arisen from the fact that the chemical names of two quite different substances, chloride of *lime* and chloride of *calcium*, are really so very much alike in sense and sound as to be very often confused, and to be even regarded as synonymous by those who are quite conversant with them.

Chloride of *lime* is the substance which is sold in commerce under the name of bleaching powder, and it is believed to generally consist of a mixture of CaO, CaH_2O_2, $CaCl_2$ and $Ca(ClO)^2$ or $(CaO)Cl_2$. When charged as completely as possible with Cl and when in its purest form it is regarded by Kolb as having the composition represented by $Ca_3H_6O_6Cl_4$, which by the action of water is decomposed as follows:

$$Ca_3H_6O_6Cl_4 = CaH_2O_2 + CaCl_2 + Ca(ClO)^2 + 2H_2O.$$

When exposed to the air the bleaching powder absorbs water, probably in proportion to the $CaCl_2$ and CaO which it contains, but it is not regarded as a deliquescent salt. At the same time it absorbs CO_2 from the atmosphere, and the calcium hypochlorite is decomposed, probably in accordance with the reaction.

$$Ca(ClO)_2 + H_2O + CO = CaCO_3 + 2HClO$$
$$\text{or } Ca(ClO)_2 + H_2O + CO_2 = CaCO_3 + H^2O + Cl_2 + O.$$

Chloride of *calcium*, on the other hand, has the formula $CaCl_2$. Its most distinguishing and characteristic property is that it is highly deliquescent; that is, it possesses the power of absorbing moisture from the atmosphere, when it is exposed to it, to such a degree as to become a liquid. So deliquescent is this substance that it is always used as the example of that property when it is defined. Brandes found that 100 pts. of it, exposed to an atmosphere saturated with moisture for ninety-six days, absorbed 124 parts of water. The atmosphere has no further effect upon it than to liquefy it.

To compare the relative absorptive powers of these two substances, I exposed watch-glasses, containing, one ordinary bleaching powder, the other chloride of calcium, side by side under a bell glass in which a vessel of water had been placed. After an exposure of three days they were weighed, and it was found that while the bleaching powder had gained 30.70 per cent. in weight, the calcium chloride had increased 60.50 per cent. The data are as follows:

	Weight taken. Grams.	Weight found. Grams.	Increase Grams.	per cent.
Calcium chloride	22.2724	36.0195	13.7471	60 50
Bleaching powder	32.9250	43.0380	10.1130	30.70

The conditions of the experiment were quite favorable to absorption of moisture by the bleaching powder, for there was necessarily but a limited supply of CO_2 in the bell glass. When it is exposed to the air the CO_2 which it absorbs forms a crust of $CaCO_3$ over its surface which impedes the absorption of moisture,

From the consideration, then, of the hygroscopic properties of these two substances it is evident that it is the chloride of *calcium* and not the chloride of *lime* which should be used as a desiccating agent for magazines, and as it is a by product in the manufacture of chlorine and of chloride of *lime* or bleaching powder it ought to be obtained very cheaply. The porous chloride which has been dried at about 200° C. is better adapted for absorbing water than the fused chloride, since the latter contains both CaO and $CaCO_3$ as a result of igniting the chloride in contact with air.

In addition to the fact that bleaching

powder is not the most efficient desiccating agent, either as regards its power or its price, it has occurred to me that, owing to certain other properties which it possesses, it might prove to be a very objectionable substance for use for this purpose.

It is known that after gunpowder has been stored for some time its initial velocity is reduced. This is held to be due to the absorption of moisture and the consequent efflorescence of the nitre. While recognizing the force of this explanation, I have surmised that there are other causes for this deterioration, and that one of them might be found in the slow oxidation of the sulphur, its conversion into sulphuric acid, the decomposition of the nitre with the formation of potassium sulphate and nitric acid, and then the further oxidation of sulphur by this nitric acid. The potassium sulphate thus formed would act, like the glass in Gale's process, or the graphite, charcoal, and so on, of Piobert and Fadéieff, for gunpowder; the silica in use for the silicated gun-cotton, or the camphor in the gum dynamite, to reduce the rate of inflammation, or of the transmission of the explosive undulations. The most satisfactory way for testing this theory would be by examining samples of fresh gunpowder for sulphuric acid, and then, after it had been exposed for some years to the incidents of storage and transportation which obtain in the service, to examine the same lot of powder again. I have not as yet had an opportunity for putting the theory to the test.

It, however, seemed probable to me that if oxidation, of the nature spoken of, could take place in the presence of air and moisture only, it would certainly be hastened by the presence of bleaching powder, since when the latter is exposed to the air the CO_2 absorbed decomposes it in accordance with the reactions given above by which chlorine or oxides of chlorine are liberated. These products in the presence of water are powerful oxidizing agents, and will consequently act more energetically than the oxygen of the air alone. To test this I arranged an apparatus so that washed CO_2 might pass into a bottle in which bleaching powder suspended in water was placed, and the washed product of this reaction was passed into a flask in which the gunpowder to be tested was suspended in water. The gunpowder taken for the test was Oriental.

Two portions of this powder were weighed, each being placed in a separate flask, and 200 cm. of distilled water added to it. Through one of these the gas from the bleaching powder was allowed to bubble for twelve hours, and then it remained standing for some time. It was exposed to the action of the gas in all for 36 hours, most of the time being in strong daylight. The other flask stood, uncorked, for the same time in another room. Both were now filtered and 100 cm. of each were taken and treated with hydrochloric acid and barium chloride. The precipitate obtained in each case was washed and ignited as for the determination of sulphuric acid. The results were as follows:

	Weight taken. grms.	Weight $BaSO_4$fd. grms.	Per cent S oxidized.
Samples exposed to air	3.4070	.0266	0.16
" " " bleaching powder,	4.0692	.4768	1.60

That is, that in the sample of gunpowder exposed to the bleaching powder, there were ten times as much sulphur oxidized as in that which was exposed to the air.

The method of experiment described above was employed because it was known that the state of solution would favor the change, and it was supposed that, under the conditions which prevail in magazines, a marked change would not be noticed except after a considerable length of time. However, an experiment was set on foot which imitated the conditions exactly. I put a quantity of bleaching powder in the bottom of a desiccator, and on the shelf above I put a weighed quantity of the Oriental superfine saltpetre powder, in the granulated, glazed state in which it is sold. The desiccator was then covered and set aside. At the end of twenty-six days I examined the powder, and was surprised to see an appearance of change on the surface of the powder granules; so I immediately dissolved in hot water, filtered and precipitated with barium chloride and hydrochloric acid. For comparison, I made another determination of the sulphates in the fresh powder. The results are as follows:

	Weight taken grams.	Weight $BaSO_4$ fd. gram.	Per cent of S oxidized.
Fresh powder.....	6.6818	.0816	0.17
Powder exposed 26 days to atmosphere of bleaching powder,.....	6.0256	.6566	1 50

It would seem to follow, from the above results, that while the chloride of *lime* is not so efficient a desiccating agent as the chloride of *calcium*, it is at the same time very objectionable, since it *may* cause a serious deterioration of the gunpowder.

I propose hereafter to examine samples of powder which have been acted upon by the gases from bleaching powder, by means of a method which I have recently devised for testing the incorporation of gun-powder, and I hope, before long, to have the honor of describing this method to you.

TERRESTRIAL DEFLECTIONS.

By Rd. Randolph, C. E.

Contributed to Van Nostrand's Engineering Magazine.

In the October number of *Van Nostrand's Magazine*, Prof Hendricks in his allusion to an article in the June number, seems to ignore a former article by the same writer in the February number, entitled *The Influence of the Earth's Rotation on Derailments*, a criticism of which by Prof. Davis was the occasion of the one to which he refers; for in the first article the centrifugal force from the earth's axis was not considered at all; the discussion being confined to the force which Prof. Hendricks says the writer failed to notice. The omission of the consideration of the centrifugal force of the earth's surface was one of the objections of Prof. Davis, which was answered by showing that this force was neutralized by the form of the spheroid of revolutions. To this Prof. Davis might have replied that this was true only of objects on the surface which had the same rotary velocity in their respective latitudes; not those which, having an independent velocity, increased or diminished this velocity of rotation. For the form of the spheroid not being sufficiently oblate for the greater velocity would not fully neutralize the tendency towards the equator; and being too much so for the lesser, gravitation towards the pole would not be fully resisted. To this fact Prof. Hendricks has incidentally called attention, and should be credited with so doing; but he fails to give a true computation of the effects of this modification, as will be shown. He is also liable to criticism for the manner in which he has treated the subject otherwise.

The first proposition in the ignored article was, that each minute horizontal plane composing the surface of a rotating sphere rotated about a normal to the surface in proportion to the sine of the latitude of the normal; and that a body passing over these planes would experience the deflective force of each in succession. This gives a clearer view of the effect of the rotation of the sphere than the first proposition of Prof. Hendricks, viz., that a vertical meridian plane deflected about each of its radii normal to the surface of the sphere in proportion to the sine of the latitude of the normal. But neither could assist any one in comprehending the remarkable proposition that this deflective force caused the body, moving over the surface in any direction, to describe a circle whose circumference is the independent velocity of the body multiplied by the time required for the meridian plane to deflect, or the horizontal plane to rotate 360 degrees. That is to say, if the independent velocity is 10 miles per hour at 30 degrees of latitude, whose sine is 0.5, and requiring 48 hours for the plane to deflect 360 degrees, then the circumference of the circle will be 480 miles. Of course the deflection which causes this circle to be described must be uniform, and therefore the body may be supposed to move along the same circle of latitude or upon the revolving cone formed by the minute horizontal planes on that circle being extended to a vertex in the axis of the sphere; but in neither case would the supposed circle be described. It is a false and misleading

representation, and will prove a stumbling block to many readers who attempt to follow the demonstration.

Although the deflective force represented by Prof. Hendricks' equation 2 is not a centrifugal force, it not being derived from any circle actually described on either fixed or moving surface, it is the same in quantity as the centrifugal force from a circle whose circumference is equal to the distance moved on the surface of the sphere during the period required for the line upon which the body moves to make a horizontal deflection of 360 degrees. Suppose a car to be passing rapidly over the center of a revolving turn-table; if friction were absent, there would be a horizontal pressure against the right hand or left hand rail as the rotation might be to the right or left, caused by the velocity along the circles of revolution being increased on one side of the center and checked on the other; both resulting in an equal pressure on the same rail. This is not a centrifugal force, but it is the same in quantity as if it were moving upon a circle whose accompanying tangent deflected at the same rate as that of the radius of the turn-table. The distance moves by the car upon the deflecting track in the horizontal plane during one rotation, is the circumference of the circle. In both cases the line upon which the body moves, the radius in one and the tangent in the other, makes a horizontal deflection at the same rate. Thus it was stated and demonstrated in the article which Prof. Hendricks has ignored. His equation 2, which should read $f = \frac{\frac{1}{289}\ \mathrm{W}\ 24\ v\ \mathrm{Sin.}\ \lambda}{2\pi\ \mathrm{R}}$ expresses it exactly, but he has not reached it by a correct philosophical reasoning.

The centrifugal force is in proportion to the square of the velocity divided by the radius in whatever manner that velocity may be produced. But Prof. Hendricks does not seem to acknowledge this where the rotary velocity in the circle of latitude has been increased by the eastern component of the independent velocity; for he estimates the centrifugal force on these two separately, taking the square of each and dividing by the radius, as if there were two separate bodies moving with these velocities respectively. The resulting force of the first, which is balanced by the form of the spheroid of revolution, being neglected, he adds to the second the centrifugal force of his imaginary circle. If this had luckily compensated for the loss resulting from not squaring the sum of the two, the actual velocity, his result would have been correct. But such is not the case.

For the sake of simplicity we will use only the variable factors which determine the centrifugal force.

Let V represent the rotary velocity, per day, of the sphere at the given latitude, and v the independent velocity. If β is the angle between the meridian and the line of the independent movement, the eastern component of it will be $v \sin \beta$. Then will the body move in the circle of latitude with the velocity of $\mathrm{V} + (v \sin \beta)$, the radius of the circle being the cosine of the latitude, λ; that of the great circle of the sphere being the unit.

The centrifugal force will then be represented by

$$\frac{(\mathrm{V} + v \sin \beta)^2}{\cos \lambda}$$

$$\frac{\mathrm{V}^2}{\cos \lambda} + \frac{2\,\mathrm{V}(v \sin \beta)}{\cos \lambda} + \frac{(v \sin \beta)^2}{\cos \lambda}.$$

The first of these three quantities is not considered, because it is balanced by the spheroidal curve. To the horizontal component of the third Prof. Hendricks adds the force expressed by his equation 2. Unless this is equal to the horizontal component of the second, his result will be erroneous. This component will be $\frac{2\mathrm{V}(v \sin \mathrm{B}) \sin \lambda}{\cos \lambda}$. The sine of an angle being equal to the cosine divided by the cotangent, let it be substituted by $\frac{\cos \lambda}{\cot \lambda}$, then the expression will become

$$\frac{2\,\mathrm{V}\,(v \sin \beta)}{\cot \lambda}.$$

Now to find the expression by the variable factors only, as before, which corresponds to equation 2, consider the cone described by a meridian tangent as developed into a plane forming the sector of a circle. The radius of this circle is $\cot \lambda$ and the arc of the sector is V, that is, the velocity per day; for it was described in one revolution. The distance passed over by the independent move-

ment during the same revolution forms an arc of the circle contemplated in equation 2. This distance is v, the independent velocity per day, and subtends the same angle as V. Therefore the radius of this circle is

$$V : v :: \cot\lambda : r = \frac{v \cot \lambda}{V};$$

and the centrifugal force is represented by

$$\frac{v^2 V}{v \cot \lambda} \text{ or } \frac{V v}{\cot \lambda}$$

which must be equal to

$$\frac{2 V v \sin \beta}{\cot \lambda}$$

if Prof. Hendrick's conclusion is correct. If the independent movement is at right angles to the meridian the sine of β becomes the unit, and the expression becomes

$$\frac{2 V v}{\cot \lambda}$$

which is double the quantity necessary to sustain the conclusion, and which is, therefore, incorrect.

The centrifugal force of the combined velocities, when the independent one is at right angles to the meridian, is all the force that can act at right angles to the line of movement, because it is the only one which can act in the plane of the meridian, and the horizontal component of this is the only force in question. But when this movement is not at right angles with the meridian, that component of the deflective force not absorbed in the centrifugal force acts at right angles to the line of movement. So that as the angle β diminishes both the second and third quantities in the expression for the centrifugal force diminish until they vanish when the movement is on the meridian, the remaining quantity being balanced by the curve of the spheroid. But as these disappear, those components of the deflective force which are at right angles to the meridian begin to act at right angles to the line of movement reaching the maximum when that is on the meridian, where equation 2 of Prof. Hendricks expresses the whole of the force in question, and where his conclusion is correct.

Now consider the case of the independent movement being to the west. The velocity is then $V - v \sin \beta$, and the centrifugal force is represented by

$$\frac{V^2}{\cos \lambda} - \frac{2 V v \sin \beta}{\cos \lambda} + \frac{(v \sin \beta)^2}{\cos \lambda}.$$

When the movement is due west the horizontal component of the second quantity becomes

$$- \frac{2 V v}{\cot \lambda}.$$

The deflective force of equation 2, when the movement is westward, is exerted towards the pole, and must, therefore, be considered a negative quantity when added to the centrifugal force, and will be represented by

$$- \frac{V v}{\cot \lambda};$$

which, as before, is only half of the quantity required to verify the conclusion.

Whatever may be the direction of the independent movement, the deflection of its line may be resolved into two components; one in the meridian and the other at right angles to it. The former reduces to zero when the movement is at right angles to the meridian where the deflective force of the independent movement is entirely absorbed in the centrifugal force. The latter is that portion of it which is not involved in the centrifugal force, and which is the entire quantity when the movement is on the meridian.

Considering the deflective force of the independent movement as unity, the meridian component is the sine of the angle β and that at right angles to it is the cosine. Therefore, the quantity representing this deflective force is to be multiplied by the cosine for the component which is not involved with the centrifugal force; which is

$$\frac{V v \cos \beta}{\cot \lambda}.$$

This is to be added to the centrifugal for eastern movements, and subtracted from it for western movements. So that the expression representing the unbalanced horizontal force acting at right angles to the line of independent movement is

$$\frac{2V v \sin \beta + (v \sin \beta)^2 + V v \cos \beta}{\cot \lambda}$$

when it is eastward, and

$$\frac{-2V\upsilon \sin \beta + (\upsilon \sin \beta)^2 - V\upsilon \cos \beta}{\cot \lambda}$$

when it is westward. Here the minus quantities represent the forces towards the pole, and the plus quantities those towards the equator.

To obtain the numerical value of these forces the velocities must be reduced to feet per second and divided by $g = 32.22$. And cot λ must be multiplied by the radius of the sphere in feet.

In the case of the pendulum there is no consideration of force involved. The question is simply the departure of the horizontal line fixed upon the surface from the plane of oscillation during one revolution of the sphere. And this is the sector developed from the cone described by the meridian tangent at the latitude of the pendulum; or $360 \times \sin \lambda$. The horizontal deflection of the fixed line corresponds to the rotation of the minute horizontal plane from which the pendulum is suspended. But the effect of this is only to twist the thread and to cause the pendulous body to rotate upon its own axis at the point of attachment to the thread. But this has no effect upon the direction of the arc through which it swings; and there being no cause for a deflection of this arc about the normal, it makes none. Therefore, the horizontal line which necessarily deflects about the normal must depart from the arc of oscillation to the full extent of the rotation of the plane.

GAS BURNERS.*

By WILLIAM SUGG, A. I. C. E.

From the "Journal of the Society of Arts."

Coal gas, as now supplied to the public, consists of hydrogen and marsh gas to the extent of about 80 per cent., and the remainder consists of luminous hydro-carbons of various qualities, with traces of sulphur in the form of disulphide.

The gas supply of London is watched over by a Commission, appointed by the Board of Trade, called gas referees, who are Professor Tyndall, Professor Vernon Harcourt, and Mr. Pole. They are empowered to prescribe the manner of verifying the illuminating power and purity of the gas, and they have also authority to act in cases of dispute in public lighting. Their prescriptions, as emanating from the only legal body of the kind in the Kingdom, are accepted as the proper methods, so that in effect, though they are appointed in London, they may be said to be gas referees for the whole kingdom. But the use that is made of gas is a matter which is altogether out of the control of gas companies and their engineers, and here the proverb about food and bad cooks may be paraphrased. The great hinderance to the improvements in the use of gas has been the general public themselves, who have not taken sufficient interest in the kinds of apparatus employed, but have, as a rule, supplied themselves with the cheapest burners, stoves and other apparatus, utterly regardless of the waste and annoyance which this system entailed; and when they have complained, they have always attributed the causes of failure to the quality or pressure of the gas, instead of ascribing it to the true cause.

In 1871, the gas referees made an examination of a number of gas burners which they had collected from various large establishments, newspaper offices, warehouses, shops and dwelling houses, and they found that those burners (samples of those generally employed by the public) would only give about one-half the light that the gas was capable of yielding per cubic foot consumed, and several of the burners tested by them gave only one-fourth of the proper light of the gas.

They say in their report to Parliament: "The economy to the public, arising from the use of good gas burners instead

* A paper read before the Mechanical Section of the British Association.

of bad ones, is so obvious as hardly to need remark. The gas-rental of London amounts annually to more than two millions sterling. Taking a very moderate estimate, upwards of one-fourth of this sum (£500,000 per annum) might be saved by the use of good burners. This is the saving which might be made in London alone; how much vaster the sum thus economized if good gas burners were to come into general use throughout England."

Now, the quantity of gas used in London last year, according to the analysis of the London Gas Companies' accounts, prepared by Mr. John Field, was, in round numbers, 20,230,000,000 cubic feet, which is equal to a bulk of mile square × 726 feet high, and its cost to the public was £2,911,000.

The result of careful trials, made with a number of burners taken lately from private houses and shops, shows that, as a rule, the amount of light obtained by the general public, from five cubic feet of gas, is less than one half of that which it is capable of giving. The iron and metal burners, of which a great many are used, give the best result in light when they are worn out, although the shape of the flame is bad. The reason why, is that in order to obtain from what is a compound of hydrogen, marsh gas and carbon, its best effect in light, the burner must be so made that the quantity of gas required to be consumed is proportionate to the size of the burner, so that it cannot exceed the maximum quantity which the burner is made for. Then the outlet of the burner itself, whether it be of the Argand or the flat-flame form, must be so arranged that the gas issues forth at a sufficiently low rate of velocity, so that it has time to get heated to a proper degree by the hydrogen and marsh gas before it comes into combustion with the oxygen of the atmosphere. When this rate of velocity is obtained in the Argand burner, the pressure at the point of ignition is almost *nil*. In flat-flame burners the pressure of the gas must be raised to a point sufficient to blow out the flame to a fan-like shape, but it must only be sufficient to do this if it is desired to obtain a good result per cubic foot of gas consumed.

One more point is of great importance in the construction of a gas burner—that is, that the gas should not be heated until it arrives at the point of ignition. The body of the chamber below the point of ignition must, therefore, be made of material which is a bad conductor of heat; so as not only to prevent the undue expansion of the gas before it arrives at the point of ignition, but also to maintain heat in the flame.

Sir Frederick Bramwell very ingeniously pointed out, some time since, that the important point in the proper combustion of gas is not so much to keep the gas cool as to keep the flame hot. The distinction is extremely subtle; but, nevertheless, a non-conducting gas-chamber performs both these important functions. If a gas-chamber made of metal or any good conductor of heat is used, then the gas becomes expanded in bulk, and the velocity of the issuing gas is greatly increased; less time being given for chemical combinations necessary to produce a proper amount of light from it. In addition to this, as Sir Frederick Bramwell has pointed out, the heat which should remain in the flame is conducted away from it into the lower fittings of the burner, where it may burn the fingers of the incautious consumer, but it is of no manner of use in the evolution of light.

You will, perhaps, say it is not possible to conceive how the flame can be kept hot without keeping the gas cool at the same time, because if the heat is to be kept in the flame, and not conducted away down the stem of the burner, the gas must be kept cool by the means employed. But, nevertheless, there is a difference in the effects produced, and the ingenious definition of Sir Frederick is scientifically accurate. As an explanation of the expansion theory, it may be stated that if one cubic foot of gas is heated to about 500° Fahr., it will occupy the same bulk that two cubic feet do at the mean temperature of the atmosphere. Now, supposing it is issuing in a cool state from the aperture of the burner at a velocity equal to one and a half miles per hour, it must then issue at the rate of three miles per hour, if the rate of consumption is equal in both cases. Therefore it has only half the time in the heated state to combine with the oxygen of the air that it had in the cooler state, and a loss of illum-

inating power is the result. Again, as regards the maintenance of the heat in the flame; supposing the chamber from which the gas issues is a conductor of heat. In this case the heat from the flame is conducted away from it down the stem of the burner, expanding the gas and leaving the flame so relatively cooled, as to require more gas to raise it to the necessary state of incandescence to allow the oxygen to combine with it in the proper proportions. In this case, as the two operations are simultaneous, it is difficult to apportion the effect produced. But that the effect is produced is proved by the following experiments, made some time since by the late Mr. F. J. Evans.

Two Argand burners were made precisely alike in every respect, except that one had a combustion chamber made in steatite, and the other in brass. The same quantity of gas was consumed through both of these, the result being that the burner with the non-conducting chamber gave more light per five cubic feet of gas consumed than the other, the proportion being as fifteen candles to thirteen. The burner with a non-conducting chamber was quite cool immediately below the chamber, while the other was so hot that it could not be touched without burning the hand. The metal burners which are now used in enormous quantities in London and the provinces become exceedingly hot, so much so as to communicate the heat to a considerable distance down the fittings. The velocity of the issue of five cubic feet per hour of gas from these burners, varies from ten to sixty miles an hour. The worn out burners generally give the lowest velocity. As a rule, the metal burners give the lowest result in light per cubic foot of gas consumed.

For example, a metal burner of the flat-flame type, which has been stated in the columns of the *Journal of Gas Lighting* to be identical with the steatite hollow-top burner, invented in 1868, gives the following results: A large-sized burner, No. 8, burning five cubic feet per hour, gave a result equal to 11.5 candles, whilst the result obtained with a like quantity of gas from a steatite burner of corresponding size, which has a non-conducting gas-chamber, was 14.6 candles, a difference in favor of the steatite of 3.1 candles, or nearly 25 per cent. more light. Another metal burner, of a size more generally in use by the public, only gave 6.2 candles for the five cubic feet, or considerably less than half the latent value of the gas, which was 16 candles.

The fact of the difference of illuminating power, with like quantities of gas, clearly shows that the two burners are not the same by any means. The statement gravely made in the columns of the *Journal of Gas Lighting* that they are identically the same, clearly shows that if this is the belief of the gas trade generally with respect to these two burners, it is no wonder that the public, who rely on the recommendations of the trade, continue still to waste their gas in the manner pointed out by the gas referees in 1870 and 1871.

As before stated, careful tests of a collection of burners, bought from different gas-fitters and iron-mongers in various towns in England, and from their recommendation, prove that the knowledge of the proper use of gas possessed by these persons is still most incomplete, and therefore, the general public continue still to burn gas in the same wasteful manner as they did when the gas referees made their report.

The remedy for this enormous waste of gas is in the hands of the public only. Gas producers, whether they are corporate bodies or public companies, are almost powerless to oppose the vested interest which derives large profits from the sale of gas burners constructed with a view to require frequent renewals. It is only fair to say that the producers of gas have always shown the greatest interest in the improved use of gas in every way; but the speculating builder and his colleague, the local plumber and gas-fitter, hold a final power for evil over the employment of gas, which, till very lately, has been paramount.

Happily for the gas interest, the general public are beginning to take a deep interest in gas, and are acquiring a great amount of information concerning it, through the numerous gas exhibitions which have been held under the auspices of gas companies and corporate gas committees throughout the kingdom, and it is to be hoped that the forthcoming exhibition at the Crystal Palace, in October next, will very largely aid the good work

of instructing the public how to use gas to the best advantage. Thus we may hope that soon the ring of interest inimical to the progress of gas will be broken through and fresh encouragement given to the inventors of improvements in the use of gas. It will be impossible, in the limits of this paper, to give you a complete idea of all the improvements which have been, and are still being made: but I propose to indicate the direction they are taking, and give you a general idea of what they are.

It has been found that a comparatively large quantity of gas, about four to six times that ordinarily used, will give a much better result from one burner per cubic foot of gas consumed than can be obtained from four or six separate burners consuming the aggregate quantity of gas equal to that consumed by the large one; and this is true of both Argand and flat-flame burners. This is by no means a new idea; it was known to Faraday and others before him. I have in my possession an old burner made many years ago; it has several rings, and a silver top drilled with very fine holes. The quantity of gas used is large—the effect is small—in fact it is more useful for boiling water than for giving light. But this old burner is a type of a large Argand of twenty or thirty years ago. They did not succeed, because, although they produce a great amount of light, it was at the cost of too much gas. Modern Argands will produce just double the amount of light for the same quantity of gas.

There is also incorporated in some of the newest burners now before the public an idea which was originated by Professor Frankland not more than ten years ago, viz., that if the air for combustion supplied to a burner be heated before it arrives at the point of ignition, a much better result per cubic foot of gas consumed can be realized. This, you will perceive, is a mode of carrying out Sir Frederick Bramwell's idea of keeping the flame hot, and undoubtedly a better result can be obtained. This burner, although it did not come much into use, has very lately been repeated, and is now being sold on the Continent. The burner is so much like Dr. Frankland's that there is no difficulty in recognizing it at once.

Of the modern Argands there are now several kinds; one is made with two or three concentric rings of flame and a glass chimney, and is made with non-conducting steatite gas chambers, and apertures permitting the gas to issue under an almost inappreciable pressure at the point of ignition, the velocity per hour being only about $1\frac{1}{2}$ miles. In this kind of burner the gas is kept cool and the flame hot. These were first used in the public lighting of Waterloo Road, in 1879, and in Waterloo Place and Queen Victoria Street.

Another kind, of newer type, is made on the theory of keeping the flame hot by making use of the products of combustion to heat the air supply. This also combines the low velocity of emission theory, and likewise heats the gas itself. It is made by inverting the flame of the burner, the heat generated by the products of combustion, being carried away by a concentric flue, fastened around the burner, through which metallic tubes convey the air necessary to produce combustion, which thus becomes heated.

A third is constructed on the principle of keeping the flame hot and the gas cool, but has besides an arrangement for admitting separate currents of cold air around the flame, for the purpose of keeping the chimney cool. It was important to observe that, although this burner does not warm the air admitted to it for promoting combustion, yet the results per cubic foot of gas consumed are stated to be as high as any of the others, showing clearly that there is no advantage in heating the gas before combustion. As to the advantage obtained by heating the air, the practical effect upon an Argand suitable for use by such ordinary unskilled labor as it usually employed to look after gaslights, has not yet been clearly demonstrated; although it is, without doubt, a great advantage to burners of the flat-flame type, because these have always too much cold air supplied to them.

The Argand, with its more complete regulation of air, and its immunity from the effect of surrounding cold air, is able to evolve from 15 to 30 per cent. more light per cubic foot of gas consumed, than can be obtained from the best flat-flame burner; but, although the general public have no objection to glass chimneys in

paraffin and other oil lamps, they do not appear to look with favor upon the general employment of glass chimneys for gas-burners, no matter how much better the result to be obtained. I need not say that the cordial support of the ring inimical to the true interests of gas is given to the public on this point, and a great deal of very strong literature condemnatory of the Argand burners has been widely disseminated. But it is impossible, in the face of the improvements which are continually being made in gas-burners of the Argand type, to believe otherwise than that they are destined to play an important part in the gaslighting arrangements of the future. The improvements in the flat-flame burners, though not producing such high results as those obtained from large Argands, are great, and concurrently with improved lanterns, have placed the lighting of the public streets on a much improved footing. Here, again, about three times the amount of light per cubic foot of gas is obtained from the use of large burners than with the old-fashioned small ones. The consumption of gas by the large ones is only equal to the aggregate consumption of four or six smaller burners. In addition to this the improvements in the reflecting tops of the lanterns make the new lamps still more effective, and may fairly be said to double the effective power. In internal lighting, the progress of gas has been very considerable of late years. Small burners for rooms have been greatly improved. For large rooms and theatres new kinds of sun-burners are made, to give three times the amount of light obtainable from the old ones, and to ventilate the buildings at the same time.

One of the greatest advantages of gas is that the heat generated by the combustion can, if properly applied, be made to do the work of ventilation, and it is in this direction that the future progress of gas-lighting lies. There are many ways of utilizing this heat; some are extremely easy to put into practice, others require more preparation. Among the simplest is the method of ventilating rooms by the fish-gill ventilator, invented by the late Goldsworthy Gurney. It consists simply in covering an opening made in the wall with strips of calico fastened across the whole by tacks put into the two upper corners of each band. The bands are made just long enough for the lower part of the superior band to cover the top part of the inferior one. When fixed properly they open like the gill of a fish, hence the name; they can be used to let in fresh air, or carry off heated air from the top of a room.

These useful and simple ventilators, if employed in rooms where gas is used, would tend greatly to the comfort of the public who require a good light, but complain of the resulting heat. They work when closed by diffusion, the heated air passing through the porous medium of the calico, and the cooler air from outside the rooms passing in without draught. For the ventilation of ball-rooms it is very easy to put into the windows a frame fitted with muslin or washed calico of half or even the full size of the window. Ventilation will thus take place by diffusion, and the draughts and danger resulting from incautiously-opened windows will be avoided. If the wind blows hard on this opening it may be protected by a loose curtain of muslin or calico hanging in front of it.

You will perceive, therefore, from what I have said, that the progress of invention in gas-lighting is great and continuous, and that in the future, if the public will only interest themselves just sufficiently to obtain a moderate amount of information on the subject of gas, they will be enabled to use it with great economy and comfort to themselves in every way. The facile manner in which gas can be employed to produce the light of a rushlight or the blaze of a thousand candles by the mere turn of the wrist, joined to the readiness with which it can be conveyed to great distances without any practical loss, will always ensure a large and growing demand for it everywhere.

But it must be remembered that its extreme adaptability renders it capable of being used with the most crude apparatus as well as the most perfect; and when we see in the public streets the blazing pipe and shovel, a rough but powerful burner of from 100 to 600 candles power, rigged up in a few minutes by a navvy, we must not be surprised at the prevalence of crude apparatus of smaller dimensions put into practice by the public, who are not aware that they can do better with a more perfect burner.

LUBRICANTS.*

Proceedings of Society of German Engineers.

LUBRICANTS, as is well known, are used for reducing the friction of the moving parts of machinery to the lowest possible degree, thereby preventing undue wear and tear, and, at the same time, obtaining the greatest possible amount of work from the machinery. Owing to the extensive introduction of mechanical power during the last thirty years, the consumption of lubricants has grown in proportion, until it is now enormous. Lubricants are supplied to us by all the three kingdoms of nature. We derive tallow and train-oil from the animal kingdom; olive oil, rapeseed oil, palm oil, and cocoa-nut oil, from the vegetable kingdoms; resin oil and intermediary oil, so to say, between the vegetable and mineral kingdom; and mineral oil pure and simple, from the mineral kingdom.

Tallow is prepared from the fat of cattle and sheep by being heated with water, sometimes with the addition of diluted sulphluric acid or caustic soda, either by the direct application of heat or by hot steam. By this treatment the cellular tissues are destroyed, and the pure fat is separated, which then settles as a layer on the surface. Tallow thus prepared forms, at an ordinary temperature, a yellowish white, pretty hard mass, which melts at a temperature of + 40° C. It contains, if not freed from acids by alkalies, besides the neutral fats, from 1 to 5 per cent. of sebacic acid. In examining tallow, care should be taken to ascertain whether it is free from cellular tissues, free from mineral acid, as well as extraneous additions (other fats, sebacic acid, mineral substances, &c.)

Train-oil, prepared from the fat of seals, &c., in a manner similar to tallow, forms, at an ordinary temperature, a liquid of a light or dark brown color and of a peculiar odour, solidifying at a temperature of from + 5° to + 15° C. As a rule it contains a large proportion (up to 5 per cent.) of sebacic acid. The same care should be observed in examining train-oil, as is recommended in the case of tallow.

Olive oil is obtained by pressure from the fruit of the olive tree, cultivated largely in the South of France and Italy. It forms a beautifully yellow liquid of a peculiar odor, and a mild taste, which begins to get thick at a temperature of + 2° C. The contents of free sebacic acid is very small in the better descriptions; inferior sorts contain about 0.5 to 1 per cent.

Rapeseed oil or rape oil is procured from the seed of the various kinds of Brassica, the seed being crushed in powerful hydraulic presses. The oil thus obtained is a yellowish-brown to brownish-green liquid, of a peculiar odor and a pungent taste, which precipitates the mucilaginous substances of the seed which have passed in pressing only after being kept in store for some time, and becomes clear by this precipitation. But even perfectly clear oil contains mucilage or albumen in chemical solution, and these can only be removed by treatment with chemicals or by heating up to 200° C. The raw oil only cleared by storing was, and is still, used for lubricating bearings, especially railway axles. The purified oil, prepared by removing from the raw oil the mucilaginous and albuminous constituents by sulphuric acid, chloride of zinc, &c., and the free sebacic acid (on the average 0.4 per cent.) by treating with alkalies, is reserved for lubricating the more delicate portions of machinery and locomotives, steam cylinders, &c. The perfectly purified rape oil, and from which all acid has been eliminated, is of a light yellow color, almost odorless, possesses a specific gravity of from 0.914 to 0.915 at 15° C., solidifies at a few degrees below zero, and melts again at about + 3° C. In testing rapeseed oil, it must be ascertained whether it possesses the correct specific weight and melting-point, and whether it is free from mucilage, sebacic and mineral acids.

Palm oil and cocoa-nut oil are obtained from the fruit of the oil and cocoa-nut palms respectively, imported in large quantities into Europe. The fruit is either crushed in hydraulic presses, or the oil is extracted by means of sulphuret of

* From a paper read by Herr Lux before the Society of German Engineers.

carbon. The two oils or fats (for at an ordinary temperature they resemble butter) are now used very little for lubricating purposes, but extensively in the manufacture of soap.

Resin oil, gained by distilling the common resin obtained from the residue left in the manufacture of oil of turpentine from turpentine, was formerly used only in the preparation of "carriage grease," mixtures of this oil with colophony, fats, &c. Refined resin oil is now procured by freeing the crude oil by means of alkalies, and subsequent bleaching, the product being a clear semi-liquid of yellow color, possessing but a slight odor, and having a high specific gravity (about 0.970). The oils are never perfectly free from organic acids, most of them containing about one per cent.

The class of mineral oils includes a number of lubricants which have nearly all a common origin with "petroleum." Crude petroleum, as it was first won in large quantities in America (since 1859), consists of an intermixture of many organic substances, which, on account of their exterior similarity to real oils, as well as for simplicity's sake, are called "oils." These oils, which may be distilled without decomposing, differ from each other by their various specific weights, as well as by their various boiling-points. For a long time the volatile ingredients only were extracted from crude petroleum, which were used for illuminating purposes, whilst little attention was paid to the heavy oils which remained after distillation. But when those residues grew in quantity, means had to be devised for utilizing them, and it was soon found that they could be used as lubricants. At first the oil was applied in its original form, as left in the alembic, in which it still contains many impurities. But now in most cases, after the more volatile oil used for lighting purposes has been distilled over (at a temperature of from 150° to 350° C.), the lubricating oil is also driven over. The latter is then purified with acids and alkalies, and a product obtained which, in consequence of its impurity, chemical constancy, and other valuable properties, appears very suitable for use as a lubricant.

As the raw mineral oil consists of a large variety of combinations related to one another, and as those combinations, according to a greater or less degree of distillation, &c., may appear in the most various mixtures, it follows that, independent of their greater or less degree of purity, the mineral oils may possess widely varying properties principally of a physical nature. Thus the color of the different mineral oils varies from light yellow to dark brownish red; their specific weights fluctuate between 0.880 and 0.920; their cohesion,—their "body," as more generally expressed,—shows itself in all grades between the consistency, for instance, of the highly liquid linseed oil, when fresh, and the semi-liquid resin oil, &c. But, however much their physical properties may vary, in their chemical nature mineral oils are closely related to each other. They all consist (of course, only real mineral oils are included) of a mixture of carburetted hydrogen, indifferent organic combinations, which possess neither acid nor basic properties. They do not decompose either at very low temperatures or at degrees of heat which far exceed those prevailing in the steam cylinders, &c., where they are employed. They do not undergo any change either on contact with the air or with water or steam; they do not attack metals, even the most easily oxydisable, such as potassium or sodium, and are as little changed or decomposed by the metals themselves.

This chemical indifference is the principal advantage possessed by mineral oils over all fat oils, whether they are of vegetable or animal origin. All those fat oils decompose in time on exposure to the air, at high temperatures, on contact with metals or their oxides, and thus destroy, sometimes more quickly, sometimes more slowly, the parts of machinery which they are intended to preserve.

Notwithstanding those great excellences which mineral oils have over fat oils, great difficulties were at first experienced in introducing lubrication with the former more generally. Convenience, attachment to custom, and want of intelligence were amongst the obstacles which had to be overcome. Mineral oils were looked upon as entirely unsuitable for lubricating machinery, and fat oils as alone possessing the specific property of a lubricant; apprehensions were raised as to the "easy inflammability" of min-

eral oils, &c. But the introduction of mineral oils for lubricating purposes was also greatly retarded by the want of sense, and partly also by the want of honesty, on the part of individual producers and dealers. Properties were claimed for the crude mineral oil only possessed by carefully purified oil; when finally the price of the lubricating oil exceeded that of the oil used for burning, part of the latter was left in the lubricant, so that, especially if great pressure took place, it was found unsuitable. Notwithstanding all this, lubrication with mineral oil has, within the short space of the last five years, made such progress that it may justly be called, not only the lubricant of the future, but that of the present day. As the mineral oils on account of their chemical properties, are far more valuable than fat oils, and, on the other hand, owing to greater variety physically, they may be adopted more readily for different purposes than fat oils, there is nothing to prevent their general introduction.

It will be gathered from the foregoing that the displacement of fat oils for lubricating machinery by mineral oils is a great technical progress. But the use of mineral oil is a great advantage also from the point of cheapness. The best mineral oils are now only half the price of fat oils. With suitable construction of the parts to be lubricated and a correct choice of the most suitable lubricating materials, the consumption of mineral oils is not greater; on the contrary, it ought to be less, as with mineral oil no hardening or thickening takes place, and thus there is no loss. Finally in lubricating with mineral oil, the parts oiled are not destroyed, but, on the contrary, preserved; whilst the destruction of machinery parts, such as pivots, regulator valve-rods of locomotives, &c., is only too often a consequence of the use of fat oils.

But the ultimate introduction of lubrication with mineral oils is of importance also from an economical point of view. Our population is steadily increasing, and the difficulties of gaining a livelihood are growing with it. On the other hand, there is an abundance of suitable material for lubricating with mineral oils, for, without reckoning the almost inexhaustible stores in America, petroleum is now found in large quantities also in Russia, Galicia, and Germany. Our food supply would be greatly extended, either directly, by appropriating large quantities of fat and oils for the maintenance of the people, or indirectly, by restoring the areas now used for the cultivation of oil-producing seeds for raising cereals. It is highly necessary in the economy of nations that there should be, not only a division of labor, but also a classification of the work to be done. Let the inferior materials, as far as they can, be used for inferior objects, whilst the more valuable products are reserved for a higher purpose.

STEEL VERSUS COMPOUND ARMOR.

From "The Engineer."

We have now a good deal of evidence before us as to the conclusions arrived at in Italy, Russia, and Denmark, on the results of the recent competitive trials made in those countries. We gave a translation of an Italian report on trials made at Spezia, subsequent to those reported by us at the end of last year. The speech of his Excellency the Minister of Marine, Rear-Admiral Acton, in the Chamber of Deputies at Rome, which we have before us, deals more fully with the entire question, and cites conclusions obtained from Russian authorities; and in the *Army and Navy Gazette* of June 16th is an article containing extracts from the Danish Ordnance Select Committee Report. These may well be taken together. Admiral Acton admits that photographs giving the views of the plates tested at Spezia might give an impression very unfavorable to the compound plates to any one who did not know the circumstances connected with the bolting; in fact, who did not know that six bolts were matched against twenty. He explains that subsequent trials on the fragments of plate with

medium guns showed a great advantage on the side of the compound plates. He then dwells on the different natures of the resistance of compound and steel plates. The former has a very hard face, which breaks up projectiles in a remarkable way, and would have a still greater advantage in this if struck obliquely. Its soft foundation plate, however, requires rigid backing, such as it did not get at Muggiano—that is, at Spezia—but such as it would have in the side of any vessel.

The steel armor is actually penetrated and breaks up under repeated blows from projectiles of medium caliber. The Admiral remarks that in preferring compound armor to steel, Italy is in good company with England, Germany, Austria, and Russia; indeed, France herself continues to employ compound as well as steel armor. Recently some magnificent Krupp steel projectiles practically produced no better results against compound armor than the Gregorini chilled iron shot. On service it is held by Admiral Acton that a ship would much more frequently be exposed to the continued fire of medium guns than to even a single shot of a really heavy gun; and compound armor resists this continued fire well, while steel succumbs to it. He would be disposed to test compound and steel plates, respectively, by one or two heavy blows and a number of medium ones. After explaining how the particular tests employed at Muggiano were arrived at, and giving a list of English, Russian, American, and other ships, for which compound armor is to be employed, supplied from the firms of Messrs. Brown and Messrs. Cammell, he quotes from the Italian officer on special service at the St. Petersburg trials of March 29th and 30th last the following words: "The tests executed on the 8th instant upon the Cammell plate—third and fourth shots—are the sequel to those of the 24th of November. The projectiles broke up upon the plate without causing practical damage to the same, so that it would serve for two or three more shots, which will probably be executed upon it;" and, again, "Admiral Schwartz, head of the Russian artillery service, and Admiral Schestakow, Minister of Marine, notwithstanding the Spezia tests in November and those made in Sweden, is strongly of opinion that the compound plates are superior to the Schneider. I thought it well to refer these points to the officials of the marine with whom I spoke at the Polygon, among whom was admiral Stahl, head of the Testing Commission, and all share the opinion of Admiral Schestakow and Admiral Schwartz, that the compound plate is superior to the Schneider." Admiral Acton then finally gives it as his opinion that "there can no longer remain any doubt as to the real superiority of compound armor which we have adopted for Italy, which England, Russia, Germany, and the other Powers continue to prefer, and which even France adopts in competition with Schneider, without arresting its manufacture." The Danish Ordnance Committee, on the other hand, argue as follows from their experiments: "The plates have suffered in proportion to the number of shot kept out. The latter being the main point, it must be acknowledged that next to the steel plates, the greatest resistance has been rendered by the iron plates, which thus have discharged their duty best; whereas the compound plates are inferior to the other sorts in both respects."

It seems a strange thing that such different apparent results should be exhibited in different countries, especially that the Danes should prefer steel plates in the teeth of conclusions arrived at by Italy and Russia after experiments conducted on a much larger scale. The fact is that experiments and results can be considered from different points and with different objects in view. Let us see what features we are sure of in the condition of the question.

Speaking generally, the compound principle appears to have a great advantage in one respect. The body of the shield requires tenacity, while the face should be hard in order to break up the shot before they get their points in, and so receive support from the plate itself. In fact, qualities of different kinds are called for in the front and back of plate. In the case of a homogeneous mass of steel the best combination or compromise of qualities must be employed. In the case of the compound plate the metal suited to each part can be there employed, that is, the shield may have a hard steel face on a tough iron foundation plate. The solid steel plate then must have a

softer face than is desirable to secure the toughness required throughout. Its advocates probably, therefore, depend on the superiority of steel over iron in tenacity, on the advantage of the whole plate being stiff and less likely to bend than the wrought iron, and on the hammering to which it is subjected in making. As we have before stated, we believe that the advantages of the hard face and tough back of compound plates are very great; but we cannot help thinking some day hard and soft steel may take the place of hard steel and soft iron. Our present business, however, is with compound and steel plates as they stand, and we are glad to find that the judgment is strongly in favor of the former in almost all cases. Apart from the actual comparison of results under a fire which is at all a match for the armor, we feel with Admiral Acton, that thick armor is much more likely on service to be fired at by guns far below its power, and with projectiles striking it obliquely; and we know that under these conditions steel gradually shatters, while iron suffers very little, to say nothing of the advantage of the hard steel face. Of course, it must be imperative that the plate cannot be punched or perforated completely. In wrought iron it becomes possible that live shell might be driven through, but this we regard as out of the question with steel-faced or steel armor. We believe, then, we are doing well with compound armor.

With regard to the experiments themselves, we do not think that they are conducted always in a sound way, admitting of comparison with each other. In a recent article on "Hard Armor"—*Engineer*, May 4th last—we pointed out that, even in the most recent experiments against hard armor which cannot be perforated, shot are always matched against plates according to their power of perforation—which is misleading when the shot are employed in work of a different character. Against hard armor, including chilled iron, steel, and the thickest kinds of steel-faced plates, the shot may either drive their points slightly into the surface, or, as in chilled iron, chip a little of the face off, but their further action is that of a hammer breaking the plates across. No hole is made, and it is difficult to conceive that "penetrating power," which is work proportioned to the size of hole which might be made in soft plates, is the proper standard of comparison. Surely the total stored-up work, delivered on the point of impact as far as the shot holds together, is the standard to employ to compare shot fired against the same plate. Probably this is the standard employed by the Danish officers. There is more difficulty in comparing experiments against plates conducted on the different scales, such as those of Spezia and St. Petersburg. We have, in the article referred to, taken the striking energy per ton of metal in the plate. This supposes that the work of cracking plates through is in proportion to their mass, which is hardly likely to be correct. Nevertheless, it is probably as near the mark as anything we can at present suggest. By this it appears that the blows recorded by us in our reports of the Spezia and St. Petersburg trials had the following work in them per ton of metal in the plate struck. At St. Petersburg, taking the plates of 12¼ tons each, the heavier blows fired first had 8704 foot-tons energy, and the lighter ones, which followed, 5228 foot-tons each, or 711 and 427 foot-tons per ton of plate. So that the Schneider which was broken up and a quarter of the plate stripped off after three blows, had received 1564 foot-tons, while the Cammell-Wilson remained good after receiving 1991 foot-tons for each ton in the plate. At Spezia the plates weighed about 31½ tons each. Consequently, the lighter and heavier blows of about 21,000 and 33,800 foot-tons, respectively, gave on the plates about 666 and 1070 foot-tons for each ton of metal in the plate. After the second round, the Cammell and Brown plates were broken and stripped. Schneider's was really broken, but held up by the backing. Each had then about 1732 foot-tons per ton of plate. Schneider received two more blows, making a total of 132,000 foot-tons, or 4,200 foot-tons per ton of plate. Bolted as it was, it behaved excellently, and although fragments were hanging by single bolts at the top, it could not be said that much more than about a quarter of the backing was exposed.

It will thus be seen that the Spezia plates were subjected to a much heavier shock of impact than those at St. Petersburg; that is, to blows of 666 and 1070

instead of those of 427 and 711. Let us compare the best compound and steel plates. The Cammell-Wilson at St. Petersburg has now, however, borne 1991 foot-tons per ton, without any complete through fracture, whereas the Schneider, at Spezia, after 1732 foot-tons, that is, after the second round, was broken through in different directions. We could hardly say how many of the cracks went entirely through it, because it was surrounded by a frame, but we could see along its edge enough to be satisfied some did so. By this mode of comparison the compound Cammell plate has already borne more than the Schneider at Spezia. In favor of the latter, however, we must urge that it is much harder to make a good 19in. plate than one only 12in. thick, and we should like to see more trials on the larger scale.

OBSERVATIONS ON THE MOUTHS OF LAKE TRIBUTARIES.

By WALTER P. RICE.

From the Journal of the Association of Engineering Societies.

While this paper may seem somewhat in the nature of a collection of isolated facts, yet they are all links in the great chain of causes governing the regimen of the mouths of lake affluents, and as such are worthy of attention.

GAUGING THE CUYAHOGA RIVER.

During March last I conducted a series of observations on the volume and currents of the Cuyahoga, under direction of Col. J. M. Wilson. These observations, although limited, afford interesting data. I used the current apparatus adopted by General Ellis. It is made of tin, and consists of two parts, the principal or submerged float being an annular ring 8 inches high and 8 inches extreme diameter, with an air space of ½ inch, making inner diameter 7½ inches; the other, or surface float, is an ellipsoid 6 inches diameter by 1½ inches depth. The two parts are connected by a copper wire .036 of an inch diameter, attached to the center of figure of the submerged position by means of two brass cross-wires 4 inches from the top. Lead is applied to overcome the flotation, and in sufficient excess to bring about 8 ounces tension on the wire.

Observers who have used this apparatus state, that it is apt to assume a hexagonal condition from effects of water pressure in depths varying from 20 to 30 feet.

The instrument which I had constructed must have been of an inferior quality of tin, for it partially collapsed under pressure at a depth of only 9 feet, and started two sides of what would constitute an almost perfect heptagon if completed.

Currents.—The river was gauged a little south of the U. S. Life-Saving Station, the bottom being very uniform, and the greatest care taken to insure accurate results. The result is embodied in the following table:

VOLUME AND CURRENTS OF CUYAHOGA AT MOUTH.

Date.	Maximum current.	Volume of water.	Remarks.
	Feet per min.	Cubic ft. per m.	
February 15, 1882.....	49.5	91,063	Wind S. W., max. vel., 19 miles.
February 20, 1882.....	168.7	360,307*	Wind N. E., max. vel., 9 miles.
March 3, 1882.....	27.1†	56,653	Wind W., max. vel., 13 miles.

* NOTE.—Rainfall of 0.8 inches, February 19.

† On March 3 the maximum velocity is only 5.4 inches per second, a velocity insufficient to hold sand in suspension.

The term mouth, as applied to a river, is most certainly a happy expression. Take any of the rivers emptying into Lake Erie, for instance, we find at the mouth respiration as in man. Irregular breathing, if we may be allowed to use the comparison, a flux and reflux of breath, currents and reverse currents at certain seasons, and, under certain conditions, pulsations as frequent and irregular as those of a sick man. As an illustration. In attempting to obtain a set of observations with regard to the discharge of the Cuyahoga, February 11, 1882, the following was the result of four observations in the central sub-section of the river:

During an interval of forty-five minutes, with a rise .075 of a foot in the water level the mid-depth velocity declined 75 per cent., the loss of velocity being 50 per cent. for the last 15 minutes of the interval. Any rise of water from winds off the lake backs the water up in the river and produces just such results. For this reason, to obtain the best results in gauging such a river, the section selected should be far enough removed to be independent of the movements of the lake.

The experiments on the Cuyahoga developed the fact of the existence of an eddy. The stream suffers a contraction at the Lake Shore & Michigan Southern Railway draw-bridge, with corresponding increase of head or velocity. After passing this point there is a decrease of velocity and consequent loss of head, and the corresponding mechanical effect is employed in working on the elements of the more slowly moving current below, and in the formation of an eddy. The abutment takes up about 25 feet of the waterway. The divergence of the currents takes place about 22 feet from the dock, and the eddy, as determined, agrees with and affords a fine practical confirmation of the laws governing the dynamics of fluids.

The submerged float being set for a depth of 9 feet was released at the mouth of the Cuyahoga, and followed the path marked out, the wind shifting between east and southeast with a velocity of ten miles an hour. After leaving the protection of the pier, the curve of the current is normal to the direction of the wind force. This shows the strong inclination of the river to flow to the eastward, even with a strong opposing force. As an approximation, the average velocity of the current over the path indicated was about 9.1 feet per minute.

DYNAMICS OF LAKE ERIE.

Under this head we shall notice the movements of the lake as all such changes of level necessarily affect all tributaries in the vicinity of their mouths.

Effect of Long-Continued Winds.—The rise and fall of the water from this cause is sometimes very great; winds blowing off shore lowering the water level, and winds blowing off the lake heaping up or raising the level. The action of the water is sometimes several hours in advance of the coming wind, and offers sure data for predictions. At the upper end of Sandusky Bay I have observed the water fall to such an extent that tugs which were in active operation on Friday, November 12th, 1880, had to be propped up on Sunday, the 14th, this being the effect of a three days' blow from the southwest, the water having fallen between four and five feet. I have noticed almost as great a fall in Maumee Bay, which is very similar in its characteristics. I have also noticed a change of level partly due to westerly wind and partly to contraction, between the river and lake at Port Clinton, within 1,000 feet of the end of the pile revetment; the water on the west or river side of the revetment being $3\frac{1}{3}$ inches higher than on the east or lake side.

Annual Fluctuation.—There is an annual fluctuation of the water level of the lake in harmony with the laws of evaporation and rainfall, high water occurring in June or July, and low water in January or February. These months seem to represent the extremes of all atmospheric phenomena, including even earthquakes. The wonderful unity of action of all these forces is shown by the series of annual profiles. The amount and character of the annual fluctuation of the lake is dependent upon the atmospheric condition.

Pulsations, or "Seiches."—Besides the decided change in water level, directly due to the action of winds on the lake, there are certain periodic pulsations or throbbings of the lake and mouths of affluents certainly not due to this cause.

We must search elsewhere for an explanation. The solution which occurred to me is as follows: Conceive Lake Erie to be the reservoir or bulb of a gigantic barometer, of which the affluents for a certain distance from the mouths constitute the stems. Here we have a most sensitive barometer, the readings of which may be taken from the water gauge. The rarity or density of the atmosphere pressing upon the lake's surface being the direct cause of the pulsations. I believe the water gauge to be the true index to this barometer, and if we had sufficient data we could determine the condition of the atmosphere from the reading of the gauge. These pulsations, as a general rule, are small. I suspect from the reason that the storm centers avoid the lake, going north or south of it; but in the case of a rapidly moving storm area over the lake I have no doubt these harmless pulsations would develop into a great change of level, and that it would be as much if not more dependent upon the varying pressure of the atmosphere than upon the attendant storm winds. I offer this as explanatory of the tidal waves sometimes seen on the great lakes. After writing the above I found Col. Whittlesey had noticed these pulsations, and he speaks of them as follows:

"There is a sudden flux and reflux, which is completed in a few seconds, or minutes; sometimes due to storms, but more often cannot be traced to any cause. These oscillations are not yet explained; they occur on all the lakes and upon other bodies of water, causing a rush into the mouths of rivers, generally of a few inches in height, but sometimes several feet."

Upon further search I find that this same phenomenon has been noticed on the Lake of Geneva and the Baltic, under the term "Seiches," and Schulten has demonstrated the direct connection between the "Seiches" of the Baltic and the height of the barometrical column.

"When pressure of air diminishes, the water begins to swell (Seichie). When barometer again rises, the surface of the sea sinks; the movements of the water are always a few minutes earlier than those of the instrument, on account of the greater mobility of the aqueous particles."

I make the above quotations as *apropos* and a confirmation of my views. It seems as if careful observations on the great lakes, with self-registering water barometer and gauge in connection, with the meteorological information obtained by the Weather Bureau, could not fail to be productive of results most satisfactory in the development of the law of storms and atmospheric disturbances.

The disturbances of water level in the lake might be classified as follows:

I. "Seiches," or water swelling.
II. Lunar tide.
III. Fluctuation directly due to wind.
IV. Annual fluctuation.
V. Secular.

Of these disturbances, I., III. and IV. are important factors as affecting the regimen of lake tributaries at their mouths.

Ice Gorge at the Mouth of the Cuyahoga.—In the spring of 1881 the ice gorged at the mouth of the Cuyahoga, flooding the docks in some places and causing great apprehension. The action of the gorge affords an interesting study. Before its formation the water possessed an average depth of 17 feet at the ends of the piers. The river, in obedience to its laws, attempts to sweep to the eastward over its accustomed course; but in this direction it must of necessity pass over its shoal, which, serving as a nucleus for the collection of ice, offers opposition, and throws the path of least resistance to the westward.

The aqueous wanderer therefore makes another turn and passes out to the westward. The latent force of the accumulated waters is dissipated by the work of cutting out a new channel.

Formation of Bars and Beach.—The general tendency of rivers along Erie's southern shore, and entering normal to the coast line, or north and south, is to sweep to the eastward in a curve more or less modified by the winds. This is due to the fact that the westerly wind is the prevailing one and to the downward flow of the lake. In the rivers of this class and of the size of the Cuyahoga, Grand, Ashtabula and others, the greatest shoal will be found in or near the prolongation of the east pier, and at distance dependent, of course, upon the volume and velocity, in figures, from 200 feet to 500 feet from end of pier. The west wind is

therefore quite an ally to the engineer, and probably a disappointment to dredgemen.

In regard to the making or advancing of shore lines, the sand collects on the windward side of the piers when exposed to the prevailing wind, or when the topography of the coast line destroys the latter to the windward, considering the wind having the greatest scope of sea.

Marshes.—Most of the tributaries of Lake Erie along the southern shore are marshy at their mouths. While these marshes at first thought seem to be of no earthly use except as active agents in the dissemination of malaria and gratification of sportsmen, yet they play a very important part in the regulation of these streams in time of flood. The marsh is to the river what the governor is to the steam-engine. They are the natural storage reservoirs for surplus water. It may be well to note the fact that the amount of evaporation is greatly increased by the spreading of this dangerous excess over the vast area of the swamp land. It is evident that the reclamation of these low lands will have the effect of raising the water level and changing the regimen of the adjacent stream.

ON A NEW METHOD OF SINKING SHAFTS IN WATERY, RUNNING GROUND.

By WILLIAM GALLOWAY.

From "Nature."

When an attempt is made to sink a shaft in very watery deposits of gravel, sand and mud in the ordinary way—that is, by digging out the solid matter by hand, and pumping the water to keep the bottom dry—it is found that, after a certain depth has been reached, the current of water which flows up through the bottom brings solid matters along with it as fast as they can be removed, and further downward progress is then completely arrested. Under these circumstances it is necessary to resort to certain special methods of sinking, two of which have been hitherto employed with more or less success. According to one of these methods the shaft-lining consists of an air-tight iron cylinder fitted with an air-tight cover. When the excavation is continued below the natural level of the water, compressed air is forced into the interior of the shaft so as to drive back the water, and leave the bottom dry. The workmen can then stand in the bottom and remove the solid matter by hand as easily as if the ground had been naturally free from water. The lining sinks downward as its lower end is laid bare, and is lengthened at the top as required. The pressure of the air is gradually augmented as the depth increases, but unfortunately this process cannot be carried beyond three atmospheres without prejudicially affecting the health of the workmen. When the depth of the watery running ground surpasses the limit represented by a pressure of three atmospheres, it is necessary to resort to the second method. In this case the water is allowed to stand at its natural level in the shaft, and the solid matters are removed from the bottom by a revolving dredger. The lining or casing consists of a cylinder of masonry or iron provided with an iron shoe or cutting ring, and sinks downwards at first in virtue of its own weight, being lengthened at the top as in the previous case, but after a time it generally becomes necessary to force it down by the pressure of screws, assisted by the blows of an instrument resembling a pile-driver. When it cannot be made to sink deeper, another similar cylinder of smaller diameter is introduced into its interior, the same series of operations are again gone through, and so on until the solid ground is reached.

Simple as the last-described process may appear, its application is sometimes attended with difficulties of almost incredible magnitude. As an example we may mention two shafts which were sunk through about 400 feet of the kind of ground in question at the Colliery Rheinpreussen, near Ruhrort in Ger-

many. One, begun in 1857, was not finished after more than eighteen years' constant perseverance; while the other, begun in February, 1867, was only completed down to the solid ground in June, 1872.

The new method invented by Herr Poetsch is described by Bergassessor G. Kohler in the *Berg und Huttenmannische Zeitung*, No. 38, xlii. *Jahrgang*, September 21, 1883. It consists in freezing the water contained in that portion of the running ground which occupies the position of the intended shaft into a solid mass of ice, and then sinking through it by hand without having to pump any water. To this end a preliminary shaft of larger dimensions than the intended shaft is sunk down to the natural level of the water. A number of vertical bore-holes, about one meter apart, are then put down round about its sides at the bottom, so that they pass through the ground just outside the lining of the intended shaft. Others are put down within the area of the intended shaft, and one is put down in its center. All of these bores are continued down to the bottom of the running ground. They are made by means of the sand-pump, and are lined with sheet-iron tubes in the usual way. A circular-distributing pipe with small copper tubes branching from it is placed at the bottom of the preliminary shaft. One copper tube extends to the bottom of each bore-hole, and each tube is provided with a stopcock. At the surface are several ice-making machines of the Carré type. The liquid intended to circulate through the bore-holes and effect the operation of freezing consists of a solution of the chlorides of magnesium and calcium, whose freezing point lies between — 35° C. and — 40° C.

By means of a small force-pump it is made to circulate at such a rate that it leaves the cooling-trough with a temperature of about — 25° C. It descends into the distributing pipe, passes through the copper tubes to the bottom of the bore-holes, ascends outside the copper tubes to the top of the bore-holes, finds its way into the collecting-tube, reascends to the surface, passes through the cooling-trough, and then commences the downward journey again.

Herr Poetsch estimates that, under ordinary conditions—that is, when the outer ring of bore-holes can be made in the ground outside the lining of the intended shaft—the freezing process will occupy from ten to fourteen days.

When it has been ascertained by means of bore-holes that the wall of ice round about the intended shaft is thick enough, the operation of sinking is commenced. The ice is cut out by hand, and a descending cylinder of masonry or iron is carried down at the same time. The lining prevents the surrounding ice-wall from breaking inwards, and the bottom cannot burst upwards.

Herr Kohler made a personal inspection of this process at the shaft Archibald, now being sunk to the lignite beds, at Schneidlingen, in Germany. The shaft passes through a bed of running sand four meters thick. Twenty-three bore-holes were employed in two rows near its sides. The freezing process was completed on August 10th last, when the running sand had become a compact mass of such great hardness that no impression could be made on it by the finger-nail, and it was with considerable difficulty that a flake 15 mm. thick could be broken from it.

Sufficient data do not yet exist for estimating the cost of this process as compared with those already known, but we are of opinion that if the operation of freezing can be effected in two or three weeks, or even months, it will compare favorably with them in this respect under almost any circumstances. We believe also that it is capable of application under a variety of circumstances not mentioned in Herr Kohler's article, such as damming back an excessive flow of water on solid ground, driving horizontal drifts or tunnels through mud and sand, and so on.

We would, therefore, recommend the inventor rather to turn his attention in this direction than to think of condensing the intake air of mines by the application of cold, with the view of dispensing with ventilating furnaces, and enabling winding operations to be carried on in upcast as well as in downcast shafts. The former field, if we mistake not, will be a large one; the latter, we can safely promise him, will be a very small one.

THE POLE COMPASS AS A GUIDE.

From the "Nautical Magazine."

In the great number of British steamers which have been lost during the last two years—one might even say *ten* years without being very far from the truth—twenty-seven per cent. have betrayed the fact most clearly that something was wrong with the courses steered. No *currents* have, in their cases, existed—unfortunately—whereon to fall back; no gales of wind to take the responsibility of their loss; no undiscovered rocks or shoals to help the bewildered out of bewilderments; nothing, in fact, but defective compasses. Well, there no doubt will be defective compasses, as long as compasses exist, just as there are defective machines everywhere; but in respect to the mariner's compass particularly, there are *degrees* of defection, which mean simple errors at one end, easily corrected, but death and destruction at the other.

If compasses are defective, and are really to blame for that large percentage already mentioned, let us see where the causes come in. A few simple inquiries into the method and system of their treatment will lead to the discovery that in the great mass of freight steamers—the great mail packet ships are better looked after in this respect—what is called the standard compass is placed upon the extreme end of a long vertical pole, somewhere in the vicinity of the bridge, and in such position, standing some twelve or fifteen feet above the level of the deck, it is supposed to be beyond all attractive influence. To be in fact beyond the attraction of beams, masts, funnel, &c., &c., of which the ship is composed. When our ships and steamers were in their infancy, when the *Great Britain*, for example, some five and thirty years ago, ran ashore on the coast of Ireland, owing, it was said afterwards to defective compasses, it was generally supposed that a compass, whose various errors had been determined by competent persons, would continue to have such errors, for a voyage or two at least, much in the same way that a fairly good watch, after being regulated for error, may be depended on to keep in that corrected condition for a month or two; or as the watchmakers say, "guaranteed for two years." But the stranding of the *Great Britain*, and other such accidents, opened the eyes of nautical experts to the fact that an error of the very best compass could not be depended on for as many days as some had given it months. A change in the course of the ship, that is to say, in the direction of her head—a heavy list to starboard or port; heavy rolling; or even a storm of thunder, with lightning—was quite enough, separately, to put the compass all wrong. And such changes were frequently sudden.

Finally, after years of trial and experiment, the experts in compass management determined to have the standard compass in such a position that it should be clear of all attraction of ship-fittings, and yet be readily got at for purposes of daily observation. In a large steamer, with spacious decks and bridges, this was not at all a difficult matter, although in smaller ships it was, yet by no means insurmountable; for, if there is room for a bridge, there is invariably also room for an azimuth compass.

Those masters of vessels who ignore the pole compass, who prefer what is called a pure and simple "azimuth compass," placed upon a pedestal on the bridge, or in some part of the ship where it is clear of attractive influence, and where observations upon it can be readily obtained, as a rule, *make* their courses shaped per standard; they make such courses because their officers are able carefully to examine the compass daily, or hourly, by sun, or stellar, or even lunar observations, at any time of day or night, as the case may be.

In the pole compass, fitted as it generally is on board freight steamers, such a system is not carried out; indeed, in very many cases, it might even be said that it is not understood. The ship when new, or on the commencement of a voyage, is swung in or near the docks by a person who makes a living by such work.

He does it correctly enough, no doubt,

and supplies the captain with a series of cards, showing what is the deviation of every point and half point of each compass on board the ship, but especially that of the pole compass. When the ship gets to sea, speaking generally, these deviations, obtained near the docks, or indeed anywhere, it matters not—change, and sometimes to such an extent as to cause much bewilderment. Yet the captain and officers, in numerous instances, are led to suppose that such deviations will hold good throughout the voyage; and in far too many cases they believe this, and act accordingly.

As an example of this system a case may be cited of a new steamer leaving the north-east coast of England for Calcutta. She was swung, on being ready for sea, in order to ascertain the various errors of the compasses. And this service was performed by a fully competent person, no doubt.

First, she was swung in the docks, where a number of iron steamers were moored, but on her captain expressing some doubt as to the value of the errors so obtained, she was taken outside the harbor some three miles from the land, and there put through another series of observations. She then returned to the docks, remained there three days, and afterwards proceeded on her voyage. The three days in the docks had been occupied in taking in a cargo in which iron, or metal of any kind formed no part. This ship made her courses good, until her head was suddenly brought to the northward, after rounding Ceylon, when in the short run from the Basses to Madras she was *forty-five miles* out of her reckoning. On leaving India and proceeding homeward, the courses made were so bad, that she was, on two occasions, within a hair's breadth of being run ashore; first, on the reefs outlying some of the islands in the Red Sea; and secondly in the English Channel. On a subsequent voyage this vessel ran ashore on the coast of Africa, and knocked her bottom out.

It must be assumed that her master and officers were as ignorant of the proper treatment of a compass, or rather of the great errors to which it is liable, as were the badly-advised owners and builders who placed such a contrivance as a pole compass on board their valuable ship. Otherwise she could never have made such extraordinary courses, which were invariably blamed to currents.

It is quite true that in the examination which masters and officers have to pass they are instructed in this branch of navigation, but like many other things, after that inquisition is ended, there is also an end to the exercise; except in mail steamers, when there is certainly more time at hand and a greater number of officers to attend to such duties. There is, however, no attempt made here to excuse officers of such vessels from carrying out such simple duties, and indeed there would be no necessity to excuse them. The fault is not so much theirs as that of the builders, or the owners, in not stipulating for an azimuth compass, on a short pedestal, in place of the long wriggling pole, whose tremors alone, when a ship is steaming, are quite enough to disorganize any compass. It is because the means are not at hand, that ships' officers and masters are negligent of such precautions. Norie, in his excellent Treatise on Navigation, published, probably, more than fifty years ago, gives the simplest and clearest instructions to officers for finding the daily, or hourly error of the compass by celestial observations. Such methods were, however, so little in practice at sea in the wooden ships, when the present masters were apprentices, that along with a great deal more in his valuable epitome, they were well nigh being consigned to oblivion.

In the various circumstances under which officers and masters are now engaged for their appointments, it is perhaps better that the ship should be thoroughly tested for compass error by some competent authority on shore before proceeding to sea; but there are no duties at sea which have a greater call upon the captain than those which demand his daily, or even hourly attention to such friendly guides; and probably there are no duties he would so readily attend to—if the means existed.

Seamen now-a-days are allowed to say so little in regard to the fitting out of ships, that it is not wonderful such a make-believe as a pole continues to exist; it has certainly a rather impressive appearance, which perhaps has something to do with its general favor amongst inexperienced builders and owners

The best place to fit a standard compass is as near the bridge as possible, if the bridge be amidships, for in the center of the ship there is less of that tremulous motion which is common to all steamers, and which has a destructive effect upon a compass's constitution. It should be at least twelve feet from all ironwork, and particularly vertical ironwork such as masts or funnels. If there is not room on or near the bridge, there are many other places, even in the smallest ships, where one could be placed with advantage. It is the fashion in some steamers, which have been built lately to stick to the pole, and to have a platform upon it, for an observer it is presumed, but such standards are not reliable, chiefly because there is too much jolting connected with them. Before concluding this paper it would be interesting to take note of the difference between the successful navigation of a ship fitted with a pole and supplied with cards of deviation by the operator who swung her, and another fitted with an ordinary azimuth compass placed upon a five-foot pedestal. In the first case the cards are examined by the captain, previous to his shaping his courses, and with a belief, firm or otherwise, in their accuracy, he steers for his intended port. Probably a good look-out by a vigilant officer discovers a light or a headland in time to avoid dangers, as was done *frequently* in the case of the steamer already cited, but when fogs and mist surround this unfortunate ship, as happened to her in her following voyage, where is she? Now, turn to one of many examples from those vessels on which daily observations are taken, and where a book is kept in which all observations are duly recorded. We shall find that in upwards of sixty per cent. of such ships in navigating either of the two channels, they make their courses, it may be truly said, to a mile, while nearly all manage it successfully and in most cases at great speed.

GERMAN RAILWAY TESTS FOR IRON AND STEEL.

From "The Engineer."

A CONTROVERSY between the Association of Ironfounders and the Union of German Railway Administrations has brought forward the test question with a certain degree of prominence in the technical press of Germany. One of the most comprehensive articles published on the subject is that which appeared in a recent number of the *Zeitung des Vereins Deutscher Eisenbahn Verwaltungen*, in which Herr Wöhler has reviewed the past history of the controversy, and has endeavored to refute the attacks on his system of classification which were made at the recent Dusseldorf meeting of the iron and steel industry. The action of the railway companies with respect to the question of classification was, it would seem, occasioned by the want of due observance by manufacturers of the needful measures for arriving at excellence in quality, and in their memorial to the Government in 1877, the companies alluded to the opposition which would probably be made by the iron and steel industry to the introduction of the new regulations then under discussion, by reason of the trouble and expense involved in making the trials and researches which would under the circumstances be rendered necessary. Since then the principles of these tests have been criticised in a hostile spirit at various assemblies of the industry affected by them, and ineffectual attempts were made at one time to get the objectionable conditions modified by the Minister of Public Works. At the meeting held at Dusseldorf, on December 10th, 1882, the principles of Herr Wohler's system were again attacked by several manufacturers, and in his brief reply, published in the *Cologne Gazette* shortly afterwards, that gentleman defended his method. It consists in measuring the resistance to fracture, and the tenacity of the metal by the contraction in the cross section after fracture. He alludes in his explanatory remarks to the difficulty of accurately measuring the extension of length at the

point of fracture, while there is no difficulty in arriving at it by means of the easily-measured contraction or diminution of the cross section. In his more detailed remarks in the technical journal already referred to, Herr Wohler alludes to the fact that if an iron or steel bar is bent by external force, it is first subjected to an elastic extension, and if the force is removed it again takes its original shape. If by the application of greater force the limits of elasticity are passed, there is then a permanent extension, the amount of which gives the measure of its tenacity, while its strength is indicated by the amount of force necessary for its fracture. These qualities are independent of each other, but if two similar bars which possess equal tenacity, but different strengths, are subjected to the same exertion, the weaker of the two will, it is considered, extend more than the stronger one, in the same way as with two bars of the same material but of different thickness, the thinner is more extended than the thicker one under an equal burden. The volume of a body is not changed by extension, and therefore a contraction is normally allied with it. Thus the extension in a longitudinal direction of a round bar causes a diminution of its cross section in the same proportion as its length increases. If the extension takes place equally through its whole length, the contraction—or diminution of cross section—is throughout alike, and the measure of extension is simply given by the difference between the original and the subsequent length, in reference to the former. This can also be given correctly by the difference between the original and the subsequent cross section, in reference to the former, and both systems of measurement must give uniform results. This is, however, not the case if the material of the bar is not equal throughout, in which case there will be an inequality in the extension corresponding to the difference which may exist in the strength of the various parts. The weaker portions extend more than the stronger, and therefore Herr Wohler argues that the extension for any particular part of the bar can only be found by measuring the contraction at each place, and not from the difference in length.

From these facts he infers that every iron or steel bar which is extended acquires in the direction of the extension a greater degree of strength. In order to extend it further the burden must be increased, and then the extension is increased until the strength has again been sufficiently augmented to allow the bar to support the increased weight. With an equal increase of the burden the corresponding increase of extension is not the same, but gradually increases, while the cross section diminishes. If the extension has reached a point where the diminution of the cross section surpasses the increase of strength arising from the greater extension, then, provided the tenacity is not exhausted, the extension can, it is true, be continued; yet the burden cannot be increased, but further extends the bar in a more rapid manner until it is broken. Even an unavoidable difference in the strength of a material suffices to produce this effect at one part of the bar somewhat earlier than in other portions. If the same trial is then made on one of the broken pieces, the effect referred to manifests itself in another portion of the bar. These facts are considered as indicating the advantages of estimating the regularity and equality of the material by the relation between the extension of the bar and the contraction. In good tough steel it is said that this proportion is approximately 1 : 2—only an approximate estimate of the tenacity can be deduced from the extension, and this is less likely to be exact according to the greater irregularity of the material.

While thus advocating the principle of taking the contraction in the cross section of fracture as indicating the tenacity of the material, Herr Wohler admits that there may be some exceptional cases in which this method is not applicable, and on which objections have been founded by the opponents of this system. He considers that the method in question displays every fault of the bar which is being tested, and thus facilitates the task of the officials in charge, of the examination of the material submitted. Though the test may present some inconvenience to one or other of the houses interested, it is argued that it is not in any way injurious to the interests of the manufacturers in general. In drawing up the conditions in question the railway companies were influenced by the desire of assuring themselves that the material

they received was of itself fit to be used for the various purposes for which it might be intended, and they based their action in the matter on the principle of keeping strictly in view, in their tests, the conditions and mechanical laws which become operative when the materials are in actual use.

In his comprehensive article, Herr Wohler enumerates the various kinds of injury and wear to which axles, tires, and rails are subject, and remarks that under normal circumstances railway material is not forcibly torn, bent, or broken, and that when such violent force is brought to bear on it, then the limit of human precaution has already been passed. On the other hand, he considers that scientific tests have to be arranged in view of those small and sometimes almost imperceptible movements which, by their frequent repetition, affect the durability of the material subjected to their influence.

The test applied by the imperial railways of Alsace-Lorraine, of which Herr Wohler is manager, in the acceptance of axles, involves the sample bar being subjected to a load of 34.92 tons per square inch of the cross section during ten minutes, without any further extension taking place during that time. If this test is withstood, the bar is subjected to a further weight until it is broken. After being broken, the cross section of fracture must not exceed 65 per cent. of the original cross section. Various facts are quoted from the records of the Alsace-Lorraine Railway Direction with a view of proving that the working of the new regulations has been in no lasting way onerous to the manufacturers interested, inasmuch as there has been since they came into force a gradual diminution in the quantity of rejected material. The following table explains this assertion more fully:

	Accepted.	Rejected.	Percentage Rejected.
Tough steel tires, 1880....	4763	196	4¼
Tough steel tires, 1881....	4461	30	
Tough steel axles, 1880..	2089	99	4¾
Tough steel axles, 1881..	3259	11	

In the instances of the rejections made in 1881, there were circumstances indicating the accidental nature of the defects by which they were occasioned. The deliveries of rails seem also to prove that the new regulations have not presented any serious difficulties to manufacturers. In 1879 a contract was made for about 100 miles of rails, which was divided between two establishments. The quality figure arrived at by the sum of the figures of strength and contraction reckoned by the German standards, and subject to certain limits in their respective proportions, was fixed at eighty-five, in accordance with the recommendations of the Salzburg Congress; but it was also stipulated that if the works delivered at least three-fourths of their respective quantities in a superior make with the quality number 90—while still maintaining a strength of at least 38.1 tons per square inch—the price for the proportion of superior rails would be increased by 3 per cent. It resulted that each establishment delivered about four-fifths of its quantity in the better quality and in accordance with the required strength. The remainder of the deliveries were even higher than 90 in quality, being above 100 in one case; but being about 2 tons under the strength fixed, were consequently not reckoned as superior to the standard. It is, however, remarked that the Salzburg Congress had fixed the standard of strength at about 32 tons. It is supposed that this increase of the quality number beyond 85 did not augment the cost of production by 3 per cent., because the works would not have delivered the better quality if there had not been some advantage for them in doing so. In his address on the subject delivered by Herr Wohler before the Verein fur Eisenbahnkunde some twelve months ago, he expressed his opinion that the deliveries made during 1881 manifested a surprising uniformity, proving that homogeneousness is increased in proportion to the improvement in quality. The manufacturers deny, however, that this improvement has been brought about by the new regulations, taking to themselves the credit of having thus raised the standard of their productions by their own independent exertions in that direction.

Herr Wohler, in his most recent com-

munication to the technical press of Germany, disputes at considerable length the assertions made at the Dusseldorf Congress last December by his opponents, and the results which were there described as having been obtained from experiments made by certain manufacturers. Further experiments would seem to be contemplated by them with a view of elucidating the different effects produced by sudden and gradual imposition of burdens for the purpose of testing. He refutes in a categorical manner the arguments deduced from experiments made as to the influence of the reduction of thickness by hammering on the effective properties of steel. He maintains that railway engineers have many difficult problems to solve, for which perfection of material is indispensable, and expresses his surprise that manufacturers should oppose such a requirement if railway companies are willing to pay for it.

The manufacturers' organization has published a letter in the *Cologne Gazette* stating that the cause of the quality of the rails alluded to being above the standard was that the application of the tests is sometimes made in a stringent manner, and under circumstances which treat insignificant defects in such a way that, as a measure of precaution, the quality is made above the standard. The part of Herr Wohler's remarks dealing with the question of the business profit which must have resulted to the manufacturers does not seem, however, to have been dealt with.

ON THE DISCHARGE AND REGULATION OF THE TIBER.

By T. MONTANARI.

Translated from "Il Politecnico" for the Institution of Civil Engineers.

In the foregoing abstract professor Nazzani describes the measurement of the Tiber by means of a current meter. The present article, published at the same time as Professor Nazzina's Paper, but independently, deals with the subject from an entirely different standpoint. The author gives no new measurements, but examines those taken by other inquirers, at different times, on the Tiber, with a view to determining a new Tibrimetrical scale (*i.e.*, a table for giving the discharge of the river when its height on the Ripetta gauge is known). There were three sets of measurements available; 1st, Benetti's, made by means of rods on June 19th, 1821, when the height on the Ripetta gauge was said to be 6.24 * meters; 2d, Canevari's, on May 16th and August 17th, 1871, the heights being 9.661 and 5.80 respectively; 3d, measurements of the maximum surface velocity only, taken by Vescovali on the 13th, 14th, 15th, and 17th of December, 1871, the heights being 12.90, 11.69, 9.08, and 8.08 respectively Upon the first of these measurements a scale was founded by Venturoli; Canevari and Vescovali each founded a scale independently upon his own measurement. The author proposes to examine them all side by side, and obtain a new and accurate scale from them. The importance of the question is shown by the fact that the flood of 1870 was variously estimated by seven different engineers at 2,500, 2,800, 3,058 (Vescovali), 3,128 (Canevari), 3,765, 4,000, and 4,575 cubic meters per second.

The author discusses the method of ascertaining the mean velocity of the river by means of the velocities of the rods, applies corrections to the results obtained by Benetti and Vescovali, and estimates the discharge, from the experiments of the former, at 232.44 cubic meters, instead of 244.06 as given by the observer; and from those of the latter at 160 cubic meters, instead of 174.50, for the discharge at the height of 5.80 meters, and 794.25 instead of 997.50, at the height of 9.665 meters.

Vescovali, having measured only the surface velocity, had to make use of an equation for finding the mean velocity, and of four given by different hydraulicians:

* This is not quite certain; it may have been 6.39, but the author adopts 6.24 as the height.

$\frac{v+0.059}{v+0.15}$ or even 0.924 given by Turrazza,

$\frac{v+2.37187}{v+3.15312}$ . . . " Prony,

0.85 . . . . . " Brunings,

$0.80 \times \frac{v+2.37187}{v+3.15312}$. " Baumgarten

he preferred the last. The author points out, however, that Baumgarten's formula was derived from a very dissimilar river (the Garonne), and also from measurements taken with a current meter: whereas those of Vescovali were taken by means of floats, and he shows that, whereas the *mean* velocity of a river should be the same, whatever instruments are used, the maximum velocity will not; current meters giving higher results than rods or floats. He then gives the coefficients to be applied to the formula of Prony, derived from the observations of various experimenters. Baumgarten gives 0.80, as above, with maximum velocities varying from 0.987 meters to 3.12 meters, the mean of seven velocities less than 1.585 giving a coefficient 0.797; the maximum velocity in Prony's experiments being 1.300. Eleven experiments with floats on the Seine at Paris give the ratio of the mean to the maximum velocity from 0.837 to 0.903, the mean velocity being 0.8557; and other experiments with floats on the same river, at Poissy, Triel and Meulan give from 0.836 to 0 892 as the ratio, or a mean of 0.8433. On the other hand, in ten experiments on the Saone, near Verdun, with a meter, giving the maximum velocity (instead of the maximum surface velocity), the ratio varied from 0.803 to 0.721, the mean being 0.758. So that, in the case of the Seine and the Soane, it appears that the ratio between the maximum velocity measured by the current meter and that measured by floats is as 1 : 0.895, and it is probable that a similar ratio would hold in other similar rivers. The author concludes that Baumgarten's coefficient should have been, not 0.80, but $\frac{0.80}{0.895}$ = to nearly 0.895. No general formula can be given which will give the mean velocity of a section when the maximum is known, as this ratio depends upon a variety of causes, the cross section of the river, its depth, inclination, &c. He compares, at some length, the formulas given by different authorities for this ratio, and for that between the mean velocity in any vertical and the maximum in the same. This investigation leads him to adopt Prony's formula with coefficient 0.874, which gives $k=0.74$ when the velocity is 1.933 meter, as was the case when Canevari's measurement was taken at the height of 9.665 on the Ripetta gauge. Applying this coefficient to the velocities and areas of Vescovali's measurements, the discharges are:

	Cubic Meters.
When the height of the Ripetta gauge is 8.08 the discharge is	535.560
When the height of the Ripetta gauge is 9.08 the discharge is	717.195
When the height of the Ripetta gauge is 11.69 the discharge is	1253.664
When the height of the Ripetta gauge is 12 90 the discharge is	1668.073

from which to derive the constants of the formula of discharge,

$$Q=aH_2+bH+c.$$

(H being the height on the gauge), which give as the nearest approximation,

$$Q=23.0291\ H^2-253.3086\ H+1091.5.$$

This equation applies to heights above 8.08, but below that it is found necessary to make use of another; and the author finds that the entire curve of the Tibrimetrical scale is composed of three arcs of reverse curves. The first, beginning at the upper part, is a parabola of the second grade, and refers to heights between 14 and 8.70 meters; it is also probably approximately correct if continued above the height of 14 meters. Below 8.70 is a curve of the third grade, which descends to 5.80, below which, again, is a short branch of a parabola of the second grade, with axis not vertical, which descends to 3.30 (the point corresponding to the bottom of the river). This curve gives, in the author's opinion, the maximum limits of discharge when the river is low, the minimum limits when it is high. He also determines another curve giving minimum limits when the water is low, maximum when it is high. These two curves are shown upon a diagram, which also gives the curves of discharge obtained by Vescovali, Canevari, and Venturoli, and also a correction of Vescovali's curve by the author, and one of Venturoli's by Possenti. The paper

goes on at great length into the discussion and justification of the author's methods of calculation.

The next point considered is the relation between rainfall and the discharge of the river. A curve is constructed which shows the duration of each different amount of discharge throughout the year, the abscissas representing hours, the ordinates the height of the river; then, by multiplying each ordinate by the discharge of the river at the corresponding height (obtained from the curve of discharge previously constructed), the mean discharge which the river sends down annually for each level is found, and a curve representing these amounts is drawn, the area of which represents the total annual discharge, which the author finds to be 9,954 millions of cubic meters, and this gives as the mean discharge per second, or *module*, 315 cubic meters. This amount distributed over the 16,132 square kilometers of the river basin above Rome is equivalent to a height of 0.617 meters of water; while the rainfall, as taken at many points in the basin since 1870, is 1 meter, or from 1822 to 1871, 0.92235 meter. Hence the mean discharge of the river is two-thirds of the rainfall.

A remarkable feature of the Tiber is the constancy of the flow, which is greater than that of any other river not fed by extensive lakes or perpetual snow, and hardly inferior to some of these latter. The cause is that the river is supplied from extensive areas of permeable oolitic and cretaceous strata, which, overlying impermeable strata, form vast subterranean reservoirs. The question of the extent to which the river is supplied from these subterranean sources is gone into at some length, and the author then proceeds to discuss the ratio $\frac{D}{P}$, in which D is the mean annual discharge of the river, and P the quantity which would have been discharged if the whole of the rainfall had passed direct into the river; that is to say, $\frac{D}{P}$ is the ratio of the discharge to the rainfall. This ratio varies greatly in different years. A table is given, showing its value as calculated by different observers, from 1822 to 1861. This table the author considers incorrect owing to various causes, and he gives another worked out by himself, and using his own Tibrimetrical scale to obtain the values of D. According to this table $\frac{D}{P}$ varies from 0.876 to 0.422. In comparison with these results he gives similar calculations made in the basin of the Garonne, which show that in the case of that river the value $\frac{D}{P}$ varied during the years 1838 to 1846 from 0.829 to 0.515. From further tables he deduces the following laws: 1st, that the ratio $\frac{D}{P}$ does not vary sensibly with the rainfall; 2d, that it increases with the discharge; 3d, that these two laws seem to apply equally to the Tiber and the Garonne. The fact that evaporation is pretty constant, whatever the rainfall may be, has an important influence on this ratio. The author considers that, for the purpose of these calculations, the year should be taken as commencing on the 1st of September, so that the discharge should, as far as possible, correspond with the rainfall of the year, which is not the case reckoning from the Ist of January, as the rain and snow of the end of one year form a material part of the discharge of the next.

In an appendix the author refers to Nazzani's measurements, which give the discharge considerably smaller than his calculations. This he attributes partly to the current meter (and particularly the kind of meter used by Nazzani) being unsuitable for measuring velocities in so turbid a river as the Tiber, partly to the effect of wind not having been allowed for, and partly to other causes. He seems to admit, however, that the problem of constructing a Tibrimetrical scale is not yet solved, but must wait for further experiments. His calculations of the ratio of discharge to rainfall, however, would not be materially modified should Nazzani's measurements prove correct.

BLASTING paper is made by J. Petry, Vienna, consisting of unsized paper coated with a hot mixture of 17 parts yellow prussiate of potash, 17 of charcoal, 35 refined saltpetre, 70 of potassium chlorate, 10 of wheat starch, and 1500 of water. After drying it is cut into strips, which are rolled into cartridges.

THE POLE COMPASS AS A GUIDE.

From the "Nautical Magazine."

In the great number of British steamers which have been lost during the last two years—one might even say *ten* years without being very far from the truth—twenty-seven per cent. have betrayed the fact most clearly that something was wrong with the courses steered. No *currents* have, in their cases, existed—unfortunately—whereon to fall back; no gales of wind to take the responsibility of their loss; no undiscovered rocks or shoals to help the bewildered out of bewilderments; nothing, in fact, but defective compasses. Well, there no doubt will be defective compasses, as long as compasses exist, just as there are defective machines everywhere; but in respect to the mariner's compass particularly, there are *degrees* of defection, which mean simple errors at one end, easily corrected, but death and destruction at the other.

If compasses are defective, and are really to blame for that large percentage already mentioned, let us see where the causes come in. A few simple inquiries into the method and system of their treatment will lead to the discovery that in the great mass of freight steamers—the great mail packet ships are better looked after in this respect—what is called the standard compass is placed upon the extreme end of a long vertical pole, somewhere in the vicinity of the bridge, and in such position, standing some twelve or fifteen feet above the level of the deck, it is supposed to be beyond all attractive influence. To be in fact beyond the attraction of beams, masts, funnel, &c., &c., of which the ship is composed. When our ships and steamers were in their infancy, when the *Great Britain*, for example, some five and thirty years ago, ran ashore on the coast of Ireland, owing, it was said afterwards to defective compasses, it was generally supposed that a compass, whose various errors had been determined by competent persons, would continue to have such errors, for a voyage or two at least, much in the same way that a fairly good watch, after being regulated for error, may be depended on to keep in that corrected condition for a month or two; or as the watchmakers say, "guaranteed for two years." But the stranding of the *Great Britain*, and other such accidents, opened the eyes of nautical experts to the fact that an error of the very best compass could not be depended on for as many days as some had given it months. A change in the course of the ship, that is to say, in the direction of her head—a heavy list to starboard or port; heavy rolling; or even a storm of thunder, with lightning—was quite enough, separately, to put the compass all wrong. And such changes were frequently sudden.

Finally, after years of trial and experiment, the experts in compass management determined to have the standard compass in such a position that it should be clear of all attraction of ship-fittings, and yet be readily got at for purposes of daily observation. In a large steamer, with spacious decks and bridges, this was not at all a difficult matter, although in smaller ships it was, yet by no means insurmountable; for, if there is room for a bridge, there is invariably also room for an azimuth compass.

Those masters of vessels who ignore the pole compass, who prefer what is called a pure and simple "azimuth compass," placed upon a pedestal on the bridge, or in some part of the ship where it is clear of attractive influence, and where observations upon it can be readily obtained, as a rule, *make* their courses shaped per standard; they make such courses because their officers are able carefully to examine the compass daily, or hourly, by sun, or stellar, or even lunar observations, at any time of day or night, as the case may be.

In the pole compass, fitted as it generally is on board freight steamers, such a system is not carried out; indeed, in very many cases, it might even be said that it is not understood. The ship when new, or on the commencement of a voyage, is swung in or near the docks by a person who makes a living by such work.

He does it correctly enough, no doubt,

and supplies the captain with a series of cards, showing what is the deviation of every point and half point of each compass on board the ship, but especially that of the pole compass. When the ship gets to sea, speaking generally, these deviations, obtained near the docks, or indeed anywhere, it matters not—change, and sometimes to such an extent as to cause much bewilderment. Yet the captain and officers, in numerous instances, are led to suppose that such deviations will hold good throughout the voyage; and in far too many cases they believe this, and act accordingly.

As an example of this system a case may be cited of a new steamer leaving the north-east coast of England for Calcutta. She was swung, on being ready for sea, in order to ascertain the various errors of the compasses. And this service was performed by a fully competent person, no doubt.

First, she was swung in the docks, where a number of iron steamers were moored, but on her captain expressing some doubt as to the value of the errors so obtained, she was taken outside the harbor some three miles from the land, and there put through another series of observations. She then returned to the docks, remained there three days, and afterwards proceeded on her voyage. The three days in the docks had been occupied in taking in a cargo in which iron, or metal of any kind formed no part. This ship made her courses good, until her head was suddenly brought to the northward, after rounding Ceylon, when in the short run from the Basses to Madras she was *forty-five miles* out of her reckoning. On leaving India and proceeding homeward, the courses made were so bad, that she was, on two occasions, within a hair's breadth of being run ashore; first, on the reefs outlying some of the islands in the Red Sea; and secondly in the English Channel. On a subsequent voyage this vessel ran ashore on the coast of Africa, and knocked her bottom out.

It must be assumed that her master and officers were as ignorant of the proper treatment of a compass, or rather of the great errors to which it is liable, as were the badly-advised owners and builders who placed such a contrivance as a pole compass on board their valuable ship. Otherwise she could never have made such extraordinary courses, which were invariably blamed to currents.

It is quite true that in the examination which masters and officers have to pass they are instructed in this branch of navigation, but like many other things, after that inquisition is ended, there is also an end to the exercise; except in mail steamers, when there is certainly more time at hand and a greater number of officers to attend to such duties. There is, however, no attempt made here to excuse officers of such vessels from carrying out such simple duties, and indeed there would be no necessity to excuse them. The fault is not so much theirs as that of the builders, or the owners, in not stipulating for an azimuth compass, on a short pedestal, in place of the long wriggling pole, whose tremors alone, when a ship is steaming, are quite enough to disorganize any compass. It is because the means are not at hand, that ships' officers and masters are negligent of such precautions. Norie, in his excellent Treatise on Navigation, published, probably, more than fifty years ago, gives the simplest and clearest instructions to officers for finding the daily, or hourly error of the compass by celestial observations. Such methods were, however, so little in practice at sea in the wooden ships, when the present masters were apprentices, that along with a great deal more in his valuable epitome, they were well nigh being consigned to oblivion.

In the various circumstances under which officers and masters are now engaged for their appointments, it is perhaps better that the ship should be thoroughly tested for compass error by some competent authority on shore before proceeding to sea; but there are no duties at sea which have a greater call upon the captain than those which demand his daily, or even hourly attention to such friendly guides; and probably there are no duties he would so readily attend to—if the means existed.

Seamen now-a-days are allowed to say so little in regard to the fitting out of ships, that it is not wonderful such a make-believe as a pole continues to exist; it has certainly a rather impressive appearance, which perhaps has something to do with its general favor amongst inexperienced builders and owners

The best place to fit a standard compass is as near the bridge as possible, if the bridge be amidships, for in the center of the ship there is less of that tremulous motion which is common to all steamers, and which has a destructive effect upon a compass's constitution. It should be at least twelve feet from all ironwork, and particularly vertical ironwork such as masts or funnels. If there is not room on or near the bridge, there are many other places, even in the smallest ships, where one could be placed with advantage. It is the fashion in some steamers, which have been built lately to stick to the pole, and to have a platform upon it, for an observer it is presumed, but such standards are not reliable, chiefly because there is too much jolting connected with them. Before concluding this paper it would be interesting to take note of the difference between the successful navigation of a ship fitted with a pole and supplied with cards of deviation by the operator who swung her, and another fitted with an ordinary azimuth compass placed upon a five-foot pedestal. In the first case the cards are examined by the captain, previous to his shaping his courses, and with a belief, firm or otherwise, in their accuracy, he steers for his intended port. Probably a good look-out by a vigilant officer discovers a light or a headland in time to avoid dangers, as was done *frequently* in the case of the steamer already cited, but when fogs and mist surround this unfortunate ship, as happened to her in her following voyage, where is she? Now, turn to one of many examples from those vessels on which daily observations are taken, and where a book is kept in which all observations are duly recorded. We shall find that in upwards of sixty per cent. of such ships in navigating either of the two channels, they make their courses, it may be truly said, to a mile, while nearly all manage it successfully and in most cases at great speed.

GERMAN RAILWAY TESTS FOR IRON AND STEEL.

From "The Engineer."

A CONTROVERSY between the Association of Ironfounders and the Union of German Railway Administrations has brought forward the test question with a certain degree of prominence in the technical press of Germany. One of the most comprehensive articles published on the subject is that which appeared in a recent number of the *Zeitung des Vereins Deutscher Eisenbahn Verwaltungen*, in which Herr Wöhler has reviewed the past history of the controversy, and has endeavored to refute the attacks on his system of classification which were made at the recent Dusseldorf meeting of the iron and steel industry. The action of the railway companies with respect to the question of classification was, it would seem, occasioned by the want of due observance by manufacturers of the needful measures for arriving at excellence in quality, and in their memorial to the Government in 1877, the companies alluded to the opposition which would probably be made by the iron and steel industry to the introduction of the new regulations then under discussion, by reason of the trouble and expense involved in making the trials and researches which would under the circumstances be rendered necessary. Since then the principles of these tests have been criticised in a hostile spirit at various assemblies of the industry affected by them, and ineffectual attempts were made at one time to get the objectionable conditions modified by the Minister of Public Works. At the meeting held at Dusseldorf, on December 10th, 1882, the principles of Herr Wohler's system were again attacked by several manufacturers, and in his brief reply, published in the *Cologne Gazette* shortly afterwards, that gentleman defended his method. It consists in measuring the resistance to fracture, and the tenacity of the metal by the contraction in the cross section after fracture. He alludes in his explanatory remarks to the difficulty of accurately measuring the extension of length at the

point of fracture, while there is no difficulty in arriving at it by means of the easily-measured contraction or diminution of the cross section. In his more detailed remarks in the technical journal already referred to, Herr Wohler alludes to the fact that if an iron or steel bar is bent by external force, it is first subjected to an elastic extension, and if the force is removed it again takes its original shape. If by the application of greater force the limits of elasticity are passed, there is then a permanent extension, the amount of which gives the measure of its tenacity, while its strength is indicated by the amount of force necessary for its fracture. These qualities are independent of each other, but if two similar bars which possess equal tenacity, but different strengths, are subjected to the same exertion, the weaker of the two will, it is considered, extend more than the stronger one, in the same way as with two bars of the same material but of different thickness, the thinner is more extended than the thicker one under an equal burden. The volume of a body is not changed by extension, and therefore a contraction is normally allied with it. Thus the extension in a longitudinal direction of a round bar causes a diminution of its cross section in the same proportion as its length increases. If the extension takes place equally through its whole length, the contraction—or diminution of cross section—is throughout alike, and the measure of extension is simply given by the difference between the original and the subsequent length, in reference to the former. This can also be given correctly by the difference between the original and the subsequent cross section, in reference to the former, and both systems of measurement must give uniform results. This is, however, not the case if the material of the bar is not equal throughout, in which case there will be an inequality in the extension corresponding to the difference which may exist in the strength of the various parts. The weaker portions extend more than the stronger, and therefore Herr Wohler argues that the extension for any particular part of the bar can only be found by measuring the contraction at each place, and not from the difference in length.

From these facts he infers that every iron or steel bar which is extended acquires in the direction of the extension a greater degree of strength. In order to extend it further the burden must be increased, and then the extension is increased until the strength has again been sufficiently augmented to allow the bar to support the increased weight. With an equal increase of the burden the corresponding increase of extension is not the same, but gradually increases, while the cross section diminishes. If the extension has reached a point where the diminution of the cross section surpasses the increase of strength arising from the greater extension, then, provided the tenacity is not exhausted, the extension can, it is true, be continued; yet the burden cannot be increased, but further extends the bar in a more rapid manner until it is broken. Even an unavoidable difference in the strength of a material suffices to produce this effect at one part of the bar somewhat earlier than in other portions. If the same trial is then made on one of the broken pieces, the effect referred to manifests itself in another portion of the bar. These facts are considered as indicating the advantages of estimating the regularity and equality of the material by the relation between the extension of the bar and the contraction. In good tough steel it is said that this proportion is approximately 1 : 2—only an approximate estimate of the tenacity can be deduced from the extension, and this is less likely to be exact according to the greater irregularity of the material.

While thus advocating the principle of taking the contraction in the cross section of fracture as indicating the tenacity of the material, Herr Wohler admits that there may be some exceptional cases in which this method is not applicable, and on which objections have been founded by the opponents of this system. He considers that the method in question displays every fault of the bar which is being tested, and thus facilitates the task of the officials in charge, of the examination of the material submitted. Though the test may present some inconvenience to one or other of the houses interested, it is argued that it is not in any way injurious to the interests of the manufacturers in general. In drawing up the conditions in question the railway companies were influenced by the desire of assuring themselves that the material

they received was of itself fit to be used for the various purposes for which it might be intended, and they based their action in the matter on the principle of keeping strictly in view, in their tests, the conditions and mechanical laws which become operative when the materials are in actual use.

In his comprehensive article, Herr Wohler enumerates the various kinds of injury and wear to which axles, tires, and rails are subject, and remarks that under normal circumstances railway material is not forcibly torn, bent, or broken, and that when such violent force is brought to bear on it, then the limit of human precaution has already been passed. On the other hand, he considers that scientific tests have to be arranged in view of those small and sometimes almost imperceptible movements which, by their frequent repetition, affect the durability of the material subjected to their influence.

The test applied by the imperial railways of Alsace-Lorraine, of which Herr Wohler is manager, in the acceptance of axles, involves the sample bar being subjected to a load of 34.92 tons per square inch of the cross section during ten minutes, without any further extension taking place during that time. If this test is withstood, the bar is subjected to a further weight until it is broken. After being broken, the cross section of fracture must not exceed 65 per cent. of the original cross section. Various facts are quoted from the records of the Alsace-Lorraine Railway Direction with a view of proving that the working of the new regulations has been in no lasting way onerous to the manufacturers interested, inasmuch as there has been since they came into force a gradual diminution in the quantity of rejected material. The following table explains this assertion more fully:

	Accepted.	Rejected.	Percentage Rejected.
Tough steel tires, 1880....	4763	196	4¼
Tough steel tires, 1881....	4461	30	¾
Tough steel axles, 1880..	2089	99	4¾
Tough steel axles, 1881..	3259	11	⅓

In the instances of the rejections made in 1881, there were circumstances indicating the accidental nature of the defects by which they were occasioned. The deliveries of rails seem also to prove that the new regulations have not presented any serious difficulties to manufacturers. In 1879 a contract was made for about 100 miles of rails, which was divided between two establishments. The quality figure arrived at by the sum of the figures of strength and contraction reckoned by the German standards, and subject to certain limits in their respective proportions, was fixed at eighty-five, in accordance with the recommendations of the Salzburg Congress; but it was also stipulated that if the works delivered at least three-fourths of their respective quantities in a superior make with the quality number 90—while still maintaining a strength of at least 38.1 tons per square inch—the price for the proportion of superior rails would be increased by 3 per cent. It resulted that each establishment delivered about four-fifths of its quantity in the better quality and in accordance with the required strength. The remainder of the deliveries were even higher than 90 in quality, being above 100 in one case; but being about 2 tons under the strength fixed, were consequently not reckoned as superior to the standard. It is, however, remarked that the Salzburg Congress had fixed the standard of strength at about 32 tons. It is supposed that this increase of the quality number beyond 85 did not augment the cost of production by 3 per cent., because the works would not have delivered the better quality if there had not been some advantage for them in doing so. In his address on the subject delivered by Herr Wohler before the Verein fur Eisenbahnkunde some twelve months ago, he expressed his opinion that the deliveries made during 1881 manifested a surprising uniformity, proving that homogeneousness is increased in proportion to the improvement in quality. The manufacturers deny, however, that this improvement has been brought about by the new regulations, taking to themselves the credit of having thus raised the standard of their productions by their own independent exertions in that direction.

Herr Wohler, in his most recent com-

munication to the technical press of Germany, disputes at considerable length the assertions made at the Dusseldorf Congress last December by his opponents, and the results which were there described as having been obtained from experiments made by certain manufacturers. Further experiments would seem to be contemplated by them with a view of elucidating the different effects produced by sudden and gradual imposition of burdens for the purpose of testing. He refutes in a categorical manner the arguments deduced from experiments made as to the influence of the reduction of thickness by hammering on the effective properties of steel. He maintains that railway engineers have many difficult problems to solve, for which perfection of material is indispensable, and expresses his surprise that manufacturers should oppose such a requirement if railway companies are willing to pay for it.

The manufacturers' organization has published a letter in the *Cologne Gazette* stating that the cause of the quality of the rails alluded to being above the standard was that the application of the tests is sometimes made in a stringent manner, and under circumstances which treat insignificant defects in such a way that, as a measure of precaution, the quality is made above the standard. The part of Herr Wohler's remarks dealing with the question of the business profit which must have resulted to the manufacturers does not seem, however, to have been dealt with.

ON THE DISCHARGE AND REGULATION OF THE TIBER.

By T. MONTANARI.

Translated from "Il Politecnico" for the Institution of Civil Engineers.

In the foregoing abstract professor Nazzani describes the measurement of the Tiber by means of a current meter. The present article, published at the same time as Professor Nazzina's Paper, but independently, deals with the subject from an entirely different standpoint. The author gives no new measurements, but examines those taken by other inquirers, at different times, on the Tiber, with a view to determining a new Tibrimetrical scale (*i.e.*, a table for giving the discharge of the river when its height on the Ripetta gauge is known). There were three sets of measurements available; 1st, Benetti's, made by means of rods on June 19th, 1821, when the height on the Ripetta gauge was said to be 6.24 * meters; 2d, Canevari's, on May 16th and August 17th, 1871, the heights being 9.661 and 5.80 respectively; 3d, measurements of the maximum surface velocity only, taken by Vescovali on the 13th, 14th, 15th, and 17th of December, 1871, the heights being 12.90, 11.69, 9.08, and 8.08 respectively Upon the first of these measurements a scale was founded by Venturoli; Canevari and Vescovali each founded a scale independently upon his own measurement. The author proposes to examine them all side by side, and obtain a new and accurate scale from them. The importance of the question is shown by the fact that the flood of 1870 was variously estimated by seven different engineers at 2,500, 2,800, 3,058 (Vescovali), 3,128 (Canevari), 3,765, 4,000, and 4,575 cubic meters per second.

The author discusses the method of ascertaining the mean velocity of the river by means of the velocities of the rods, applies corrections to the results obtained by Benetti and Vescovali, and estimates the discharge, from the experiments of the former, at 232.44 cubic meters, instead of 244.06 as given by the observer; and from those of the latter at 160 cubic meters, instead of 174.50, for the discharge at the height of 5.80 meters, and 794.25 instead of 997.50, at the height of 9.665 meters.

Vescovali, having measured only the surface velocity, had to make use of an equation for finding the mean velocity, and of four given by different hydraulicians:

* This is not quite certain; it may have been 6.39, but the author adopts 6.24 as the height.

$\frac{v+0.059}{v+0.15}$ or even 0.924 given by Turrazza,

$\frac{v+2.37187}{v+3.15312}$ . . . " Prony,

0.85 . . . . . " Brunings,

$0.80 \times \frac{v+2.37187}{v+3.15312}$. " Baumgarten

he preferred the last. The author points out, however, that Baumgarten's formula was derived from a very dissimilar river (the Garonne), and also from measurements taken with a current meter: whereas those of Vescovali were taken by means of floats, and he shows that, whereas the *mean* velocity of a river should be the same, whatever instruments are used, the maximum velocity will not; current meters giving higher results than rods or floats. He then gives the coefficients to be applied to the formula of Prony, derived from the observations of various experimenters. Baumgarten gives 0.80, as above, with maximum velocities varying from 0.987 meters to 3.12 meters, the mean of seven velocities less than 1.585 giving a coefficient 0.797; the maximum velocity in Prony's experiments being 1.300. Eleven experiments with floats on the Seine at Paris give the ratio of the mean to the maximum velocity from 0.837 to 0.903, the mean velocity being 0.8557; and other experiments with floats on the same river, at Poissy, Triel and Meulan give from 0.836 to 0 892 as the ratio, or a mean of 0.8433. On the other hand, in ten experiments on the Saone, near Verdun, with a meter, giving the maximum velocity (instead of the maximum surface velocity), the ratio varied from 0.803 to 0.721, the mean being 0.758. So that, in the case of the Seine and the Soane, it appears that the ratio between the maximum velocity measured by the current meter and that measured by floats is as 1 : 0.895, and it is probable that a similar ratio would hold in other similar rivers. The author concludes that Baumgarten's coefficient should have been, not 0.80, but $\frac{0.80}{0.895}$ = to nearly 0.895. No general formula can be given which will give the mean velocity of a section when the maximum is known, as this ratio depends upon a variety of causes, the cross section of the river, its depth, inclination, &c. He compares, at some length, the formulas given by different authorities for this ratio, and for that between the mean velocity in any vertical and the maximum in the same. This investigation leads him to adopt Prony's formula with coefficient 0.874, which gives $k=0.74$ when the velocity is 1.933 meter, as was the case when Canevari's measurement was taken at the height of 9.665 on the Ripetta gauge. Applying this coefficient to the velocities and areas of Vescovali's measurements, the discharges are:

	Cubic Meters.
When the height of the Ripetta gauge is 8.08 the discharge is	535.560
When the height of the Ripetta gauge is 9.08 the discharge is	717.195
When the height of the Ripetta gauge is 11.69 the discharge is	1253.664
When the height of the Ripetta gauge is 12 90 the discharge is	1668.073

from which to derive the constants of the formula of discharge,

$$Q=aH_2+bH+c.$$

(H being the height on the gauge), which give as the nearest approximation,

$$Q=23.0291\ H^2-253.3086\ H+1091.5.$$

This equation applies to heights above 8.08, but below that it is found necessary to make use of another; and the author finds that the entire curve of the Tibrimetrical scale is composed of three arcs of reverse curves. The first, beginning at the upper part, is a parabola of the second grade, and refers to heights between 14 and 8.70 meters; it is also probably approximately correct if continued above the height of 14 meters. Below 8.70 is a curve of the third grade, which descends to 5.80, below which, again, is a short branch of a parabola of the second grade, with axis not vertical, which descends to 3.30 (the point corresponding to the bottom of the river). This curve gives, in the author's opinion, the maximum limits of discharge when the river is low, the minimum limits when it is high. He also determines another curve giving minimum limits when the water is low, maximum when it is high. These two curves are shown upon a diagram, which also gives the curves of discharge obtained by Vescovali, Canevari, and Venturoli, and also a correction of Vescovali's curve by the author, and one of Venturoli's by Possenti. The paper

goes on at great length into the discussion and justification of the author's methods of calculation.

The next point considered is the relation between rainfall and the discharge of the river. A curve is constructed which shows the duration of each different amount of discharge throughout the year, the abscissas representing hours, the ordinates the height of the river; then, by multiplying each ordinate by the discharge of the river at the corresponding height (obtained from the curve of discharge previously constructed), the mean discharge which the river sends down annually for each level is found, and a curve representing these amounts is drawn, the area of which represents the total annual discharge, which the author finds to be 9,954 millions of cubic meters, and this gives as the mean discharge per second, or *module*, 315 cubic meters. This amount distributed over the 16,132 square kilometers of the river basin above Rome is equivalent to a height of 0.617 meters of water; while the rainfall, as taken at many points in the basin since 1870, is 1 meter, or from 1822 to 1871, 0.92235 meter. Hence the mean discharge of the river is two-thirds of the rainfall.

A remarkable feature of the Tiber is the constancy of the flow, which is greater than that of any other river not fed by extensive lakes or perpetual snow, and hardly inferior to some of these latter. The cause is that the river is supplied from extensive areas of permeable oolitic and cretaceous strata, which, overlying impermeable strata, form vast subterranean reservoirs. The question of the extent to which the river is supplied from these subterranean sources is gone into at some length, and the author then proceeds to discuss the ratio $\frac{D}{P}$, in which D is the mean annual discharge of the river, and P the quantity which would have been discharged if the whole of the rainfall had passed direct into the river; that is to say, $\frac{D}{P}$ is the ratio of the discharge to the rainfall. This ratio varies greatly in different years. A table is given, showing its value as calculated by different observers, from 1822 to 1861. This table the author considers incorrect owing to various causes, and he gives another worked out by himself, and using his own Tibrimetrical scale to obtain the values of D. According to this table $\frac{D}{P}$ varies from 0.876 to 0.422. In comparison with these results he gives similar calculations made in the basin of the Garonne, which show that in the case of that river the value $\frac{D}{P}$ varied during the years 1838 to 1846 from 0.829 to 0.515. From further tables he deduces the following laws: 1st, that the ratio $\frac{D}{P}$ does not vary sensibly with the rainfall; 2d, that it increases with the discharge; 3d, that these two laws seem to apply equally to the Tiber and the Garonne. The fact that evaporation is pretty constant, whatever the rainfall may be, has an important influence on this ratio. The author considers that, for the purpose of these calculations, the year should be taken as commencing on the 1st of September, so that the discharge should, as far as possible, correspond with the rainfall of the year, which is not the case reckoning from the Ist of January, as the rain and snow of the end of one year form a material part of the discharge of the next.

In an appendix the author refers to Nazzani's measurements, which give the discharge considerably smaller than his calculations. This he attributes partly to the current meter (and particularly the kind of meter used by Nazzani) being unsuitable for measuring velocities in so turbid a river as the Tiber, partly to the effect of wind not having been allowed for, and partly to other causes. He seems to admit, however, that the problem of constructing a Tibrimetrical scale is not yet solved, but must wait for further experiments. His calculations of the ratio of discharge to rainfall, however, would not be materially modified should Nazzani's measurements prove correct.

BLASTING paper is made by J. Petry, Vienna, consisting of unsized paper coated with a hot mixture of 17 parts yellow prussiate of potash, 17 of charcoal, 35 refined saltpetre, 70 of potassium chlorate, 10 of wheat starch, and 1500 of water. After drying it is cut into strips, which are rolled into cartridges.

THE REFORESTING OF MOUNTAINS.

By P. DEMONTZEY.

From "La Nature," for Abstracts of the Institution of Civil Engineers.

THE state of sterility of the mountain slopes of the Alps, the Pyrenees, and the Cevennes, is traced to the removal of the forests which used to cover these mountains. The unrestrained torrents have gradually stripped the soil from the slopes, which, in the absence of trees, is deprived of its source of renewal, so that each year the area of vegetation becomes more restricted, and the rapid descent of the floods spreads devastation in the fertile valleys below. The first tentative measures for remedying this disastrous condition were undertaken in 1860, by encouraging the growth of forests in various mountain districts in the south of France, and the experiments have resulted in converting desolate regions into tracts covered with young forests, and in changing formidable torrents into harmless streams. In order to promote the growth of trees, it is necessary both to cover the denuded rocky slopes with a covering of softer ground, into which the roots can penetrate, and also to protect the young vegetation from being washed away by the torrent. These objects are accomplished by erecting numerous dams at suitable spots across the bed of the torrent, some constructed of masonry, designed to be permanent, and others merely temporary constructions formed with hurdles and fascines, and by protecting and regulating the sides and bed of the torrent by lines of hurdles and brushwood. The dams and cross lines of hurdles reduce the slope and increase the width of the bed of the torrent, and consequently diminish the velocity of flow, and cause an accumulation of detritus; whilst the hurdles along the banks protect the side slopes from scour, and the accretions diminish their declivity. The growth of forests is thereby rendered feasible, which in their turn protect the slopes from denudation, check the descent of the small streams into the torrent, and, by the fall of their leaves, furnish a supply of soil for promoting vegetation. The kind of trees that should be planted depends upon the site, the exposure, and the altitude; but whereas, under the recent unfavorable conditions, the limit of altitude of the growth of forest trees was placed at about 6,500 feet, pine firs and larches, of from ten to fifteen years' growth, may now be seen covering large tracts of ground at an altitude of about 9,000 feet, where the old forests do not extend beyond the original limit assigned of 6,500 feet. The result of the above kind of work, carried out in the valley of St. Bernard, near Barcelonnette, has been to retain all the detritus in the valley, so that nothing but pure water passes away. The slopes are, in consequence, becoming flatter, and it only remains to facilitate the growth of trees higher up the banks by promoting the extension of the covering of soil, which is rapidly effected by layers of willows along the banks, and even occasionally across the bed of the stream, producing a network of roots and layers, increasing with the growth of the plants. In the case of some strata traversed by numerous ravines, the accumulation of detritus would be too slow to yield satisfactory results, and it becomes necessary, after constructing a dam at the base of the ravine, to remove all the projecting portions of the slopes, and throw them into the bed of the stream, so as to raise the bed at least 3 to 6 feet. A series of lines of fascines are then placed across the new bed to maintain it, and the upper layer of rocky debris is converted, by exposure to the atmosphere, into soil suitable for some kinds of vegetation. A law was passed in France, in 1882, for replanting forests and preserving them on the mountains, and the State has undertaken the execution of the work in the most important districts. It is anticipated that the results of these works will be, security of the population and land from inundation, by the regulation of the torrents; prevention of detritus from filling up the beds of the rivers below; extension of land suitable for cultivation; increase in the discharge of rivers, and consequent possibility of extending irrigation; and a large development in the supply of wood.

THE WINDING OF ELECTRO-MAGNETS.

From "English Mechanic and World of Science."

In the current number of the *Philosophical Magazine*, Profs. Ayrton and Perry have a paper in which they describe the results of some experiments to determine which mode of winding a given length of wire on an iron bar gave the strongest electro-magnet for the same current. Four bars of iron, each 12 in. long, were cut from the same rod $\frac{3}{8}$ in. thick, and an exactly equal length of wire was wound on the four bars respectively, in the following way: 1. Wire wound equally over the whole length. 2. Wire coned towards each end. 3. Wire wound equally over half the iron bar, leaving the other end bare. 4. Wire wound on one half but coned towards the end. Electro-magnet No. 1 was put on so that its axis was at right angles to the axis of a small magnetic needle and passed through the point of suspension of the needle, which was suspended so as to move freely in a horizontal plane, and far enough away that the magnetic field due to the electro-magnet No. 1, when magnetized by passing a current through it, was nearly constant over that portion of the field in which the little suspended needle moved when deflected. A constant current was now passed through the coil on No. 1, and the deflection of the little needle observed when No. 1 was placed at different distances from the center of the test needle, the axis of No. 1, however, always remaining in the same line. Under these circumstances it is well known that the strength of the field produced by No. 1 at the center of the test-needle is approximately proportional to the tangent of its deflection. Experiments were now made in a similar way with electro-magnet No. 2, and with each end of No. 3 and of No. 4 the same current as was employed with electro-magnet No. 1 being used in all cases, and which was much below the saturating current. It was found that the effect of coning the wire was to produce a strong field very near the pole, but that the force fell off very rapidly as the distance from the pole increased. The uniformly-coiled magnet No. 1 was found to produce the most powerful field at considerable distances from the end of the electro-magnet, while for points nearer the magnet, but still at a distance of about 3 in. from it, the covered end of No. 3 magnet produced the strongest field, the next strongest being produced by magnet No. 2 with the wire coned towards each end. To ascertain the force which each magnet would exert on an armature, experiments were made, and the following results obtained, the current flowing through the coil in each case being exactly the same, as well as the armature employed:

Magnet.	Weight required to detach the armature from the covered end of the magnet.
No. 1	45 ounces.
" 2	57 "
" 3	57 "
" 4	77 "

These results confirmed the previously-ascertained fact that the field produced by the covered ends of the electro-magnets numbers 2 or 3 at distances near the pole was much stronger than that produced by No. 1. They also showed that for very small distances the covered end of No. 4 produced the strongest field. From these experiments, Messrs. Ayrton and Perry conclude that with a definite iron core, a definite length of wire to be coiled on it, and to be traversed by a definite current, the mode of coiling to produce the largest field depends entirely on the distance from the end of the electro-magnet at which the field is to be produced. With the particular magnet employed by them it is seen that, at distances from the end of the magnet—very small compared with the length of the core, the wire should be coiled up at the near end of the core; to create a field at a distance from the end of the magnet equal to about a third of the length of the magnet, it is better to coil the wire uniformly over one-half of the core than to cone it up at the near end; while for distances from the end of the magnet, equal to, or greater than, about $\frac{1}{3}$ of the length of the core, the uniform mode of winding is the best.

THE ELECTRO-MOTIVE FORCE AND RESISTANCE OF "BUNSEN" CELLS.

By GEO. G. GROWER.

Written for Van Nostrand's Engineering Magazine.

It is frequently required to know the current which can be obtained from "Bunsen" cells, on circuits of various resistances. This can easily be calculated from Ohm's law, when the electromotive force and resistance are known. Of course the external resistance must also be known.

The electromotive force of cells, as given in various books, seems very irregular; different figures being given by different authorities. This may be partly due to the fact that the electromotive force is constantly changing while the cell is working, being highest at first, and falling more or less rapidly according to the work the cell is doing.

The density of the acids or solutions used also affects the electromotive force, and as experimenters use different densities, this may also account for part of the irregularities mentioned.

The resistance of the cells keeps changing also, and very few books give any figures in regard to it. The electromotive force of cells is usually given in volts, and although the volt has an absolute value theoretically, it is practically a very uncertain unit.

The "Daniel" cell, charged with a solution of zinc sulphate, is frequently used as a standard of electromotive force; it is taken as 1.079 volt by many experimenters, but some regard it as .955 volt, and still others as one volt. This shows how uncertain the volt is as a practical unit of electromotive force.

The "Bunsen" cell is charged in a number of different ways, each of which has its advantages and disadvantages. The two commonest ways of charging is to fill the porous cup with nitric acid, or a solution of bichromate of potash in sulphuric acid. The zinc is put in dilute sulphuric acid.

To compare the merits of the two ways, one cell was set up charged each way, and they were left to run down on a resistance of about 1 ohm. The electromotive force, resistance and current were taken at intervals. The method of obtaining the electromotive force, etc., was as follows:

The cells were left short-circuited on a resistance of 1.05 ohms, except when deflections were taken. A key was inserted in line, which kept the circuit closed, except when depressed, when it threw the cell direct on the galvanometer. By means of a switch the galvanometer could be connected to the terminals of 1 ohm in the line.

The galvanometer used was a high resistance reflector shunted so that a suitable portion of the current would go through it. A high resistance was also inserted in the galvanometer line.

The steady deflections are proportional to the current, but as it takes time to bring the spot to rest, the battery may have changed considerably when the deflection is obtained. The electromotive force of the cells begins to increase the moment the current is stopped.

The various swings or impulses are also proportional to the current, so to make the observations rapidly, before changes can occur in the cell, the first swings were taken.

For a standard cell I used a "Daniel," containing a saturated solution of copper-sulphate and a solution of zinc sulphate, prepared by dissolving 1 part of zinc sulphate in 10 of water. Pure chemicals were used in this cell. The zinc was not amalgamated. The galvanometer was set so that the standard cell gave a first swing of 100, therefore each deflection meant .01 of the standard cell.

It will be seen from the above that the deflection of the cell equals the electromotive force, that off the ohm equals the current. The electromotive force divided by the current equals the total resistance; subtracting from this the external resistance 1.05, we have left the resistance of the cell in ohms.

The dimensions of the cells were:

glass jar, 10 cm. diameter, 12.5 cm. high; porous cups, 5 cm. diameter, and zincs, 6.5 cm. diameter. The carbons were 2x1.5 cm.

Each jar contained 400 cu. cm. of dilute sulphuric acid (1 to 12 by volume). No. 1 contained 100 cu. cm. of nitric acid (sp. gr., 1.35) in the porous cup; No. 2, 100 cu. cm. of chromic solution, prepared by dissolving 30 g. of bichromate of potash in 240 cu. cm. of water, and then adding 50 cu. cm. of sulphuric acid. The zincs were amalgamated the day before using; they were also weighed, so that the amount of local action could be ascertained. The cells were left charged 1¾ hours before using. External resistance was 1.05 ohm.

The electromotive force is given in terms of the "Daniel" cell; the resistance in ohms. The current answers the formula $\frac{E}{R}$.

After Working.	No. 1, Nitric Acid.			No. 2, Chromic Solution.		
	E. M. F.	Current.	Resist.	E. M. F.	Current.	Resist.
	1.71	1.21	.35	1.83	1.22	.45
5 minutes.	1.70	1.21	.34	1.40	.90	.50
10 " .	1.69	1.21	.33	1.32	.86	.48
15 " .	1.69	1.20	.34	1.29	.86	.45
30 " .	1.69	1.21	.33	1.24	.82	.46
45 " .	1.68	1.21	.33	1.20	.79	.47
1 hour...	1.69	1.23	.32	1.17	.79	.43
1½ " ...	1.68	1.24	.29	1.13	.76	.44
2 " ...	1.69	1.25	.29	1.10	.74	.43
3 " ...	1.69	1.26	.28	.99	.67	.42
4 " ...	1.68	1.26	.27	.90	.60	.45
5 " ...	1.65	1.23	.28	.76	.49	.50
6 " ...	1.64	1.22	.28	.61	.37	.60
7 " ...	1.64	1.21	.29	.48	.29	.60
8 " ...	1.64	1.21	.29	.43	.25	.67
9 " ...	1.63	1.20	.29	.41	.23	.73
11 " ...	1.60	1.17	.30	.35	.20	.70
22 " ...	1.45	.91	.53	.31	.17	.77
23 " ...	1.36	.83	.46	.31	.17	.77
24 " ...	1.17	.73	.54	.31	.17	.77
25 " ...	1.02	.63	.56	.31	.17	.77
26 " ...	.90	.55	.57	.31	.17	.77
29 " ...	.43	.25	.66	.31	.17	.77
30 " ...	.42	.24	.69	.31	.17	.77
31 " ...	.41	.23	.72	.31	.17	.77

The current here taken will dissolve in 1 hour 1.32 grains of zinc.

Zinc dissolved in cell No. 1=48.241 g.
" " by current=37.026 g.
" " by local action=11.215 g.

Zinc wasted is about 30 per cent. as much as utilized.

Zinc dissolved in cell No. 2=13.226 g.
" " by current=11.128 g.
" " by local action=2.098 g.

Zinc wasted is about 19 per cent. as much as utilized.

The above figures show that although the electromotive force of the cell with the chromic solution is at first higher than with nitric acid, it immediately falls far below it.

The nitric acid cell holds its electromotive force well until nearly exhausted. With nitric acid the resistance is less and the current stronger and steadier than with chromic solution.

The waste of zinc due to local action appears to be more with nitric acid than with chromic solution.

The nitric acid cell is far superior to the chromic as far as the work is concerned, and if it were not for the disagreeable and unhealthy fumes it gives off, would be used in nearly every case.

CENTRIFUGAL FORCE.—Professors Ayrton and Perry exhibited at the last meeting of the Physical Society an ingenious lecture apparatus for demonstrating the laws of centrifugal force. As was properly pointed out by Professor Ayrton, the ordinary lecture apparatus of this kind do not really demonstrate the laws of the subject, but simply show the effect; and a new and more scientific class of apparatus is demanded by the extension of scientific teaching. Professor Perry and he have been engaged in designing new apparatus to meet the wants of their City Guilds students, and the apparatus shown was one of the instruments in question. It consists of a rotating vertical axis carrying an aneroid chamber filled with mercury, which also rises in a graduated capillary tube projecting from its middle. A metal arm projects at right angles from the aneroid or diaphragm side of this chamber, and carries a sliding weight which can be shifted to different distances on the graduated arm. On rotating the axis the centrifugal force of the projecting arm pulls on the elastic diaphragm of the mercury chamber, and the mercury within it having more room sinks in the capillary tube by a corresponding number of degrees. The apparatus is capable of demonstrating the law of centrifugal force with accuracy, according to experiments which have been made; and as Professor Guthrie remarked it could be used for indicating the speed of wheels and shafts. We may add that there is already a mercury counter in existence, in which a closed mercury chamber is rotated, and the parabolic concavity given to the mercury by the centrifugal force is employed to measure the speed.—*Engineering.*

ON TELPHERAGE.*

By Prof. FLEEMING JENKIN, LL.D., F.R.S.

From "The Electrician."

"The transmission of vehicles by electricity to a distance, independently of any control exercised from the vehicle, I will call Telpherage." These words are quoted from my first patent relating to this subject. The word should, by the ordinary rules of derivation, be telephorage, but as this word sounds badly to my ear, I ventured to adopt such a modified form as constant usage in England for a few centuries might have produced, and I was the more ready to trust to my ear in the matter, because the word telpher relieves us from the confusion which might arise between telephore and telephone when written.

I have been encouraged to choose telpherage as the subject of my address by the fact that a public exhibition of a telpher line, with trains running on it, will be made this afternoon for the first time.

You are, of course, all aware that electrical railways have been run, and are running with success in several places. Their introduction has been chiefly due to the energy and invention of Messrs. Siemens. I do not doubt of their success and great extension in the future—but when considering the earliest examples of these railways in the spring of last year, it occurred to me that in simply adapting electric motors to the old form of railway and rolling stock, inventors had not gone far enough back. George Stephenson said that the railway and locomotive were two parts of one machine, and the inference seemed to follow that when electric motors were to be employed a new form of road and a new type of train would be desirable.

When using steam we can produce the power most economical in large engines, and we can control the power most effectually and most cheaply when so produced. A separate steam engine to each carriage, with its own stoker and driver, could not compete with the large locomotive and heavy train; but these imply a strong and costly road and permanent way. No mechanical method of distributing power, so as to pull trains along at a distance from a stationary engine, has been successful on our railways; but now that electricity has given us new and unrivalled means for the distribution of power, the problem requires reconsideration.

With the help of an electric current as the transmitter of power, we can draw off, as it were, one, two, or three horse-power from a hundred different points of a conductor many miles long, with as much ease as we can obtain 100 or 200 horse-power at any one point. We can cut off the power from any single motor by the mere break of contact between two pieces of metal; we can restore the power by merely letting the two pieces of metal touch; we can make these changes by electro-magnets with the rapidity of thought, and we can deal as we please with each of one hundred motors without sensibly affecting the others. These considerations led me to conclude, in the first place, that when using electricity we might with advantage subdivide the weight to be carried, distributing the load among many light vehicles following each other in an almost continuous stream, instead of concentrating the load in heavy trains widely spaced, as in our actual railways. The change in the distribution of the load would allow us to adopt a cheap, light form of road. The wide distribution of weight entails many small trains in substitution for a single heavy train; these small trains could not be economically run if a separate driver were required for each. But, as I have already pointed out, electricity not only facilitates the distribution of power, but gives a ready means of controlling that power. Our light, continuous stream of trains can, therefore, be worked automatically, or managed independently of any guard or driver accompanying the train—in other words, I could arrange a self-acting block, preventing collisions. Next came the ques-

* Introductory address delivered to the Class of Engineering, University of Edinburgh.

tion, What would be the best form of substructure for the new mode of conveyance? Suspended rods or ropes, at a considerable height, appeared to me to have great advantages over any road on the level of the ground; the suspended rods also seemed superior to any stiff form of rail or girder supported at a height. The insulation of ropes with few supports would be easy; they could cross the country with no bridges or earthworks; they could remove the electrical conductor to a safe distance from men and cattle; cheap small rods employed as so many light suspension bridges would support in the aggregate a large weight. Moreover, I considered that a single rod or rail would present great advantages over any double rail system, provided any suitable means could be devised for driving a train along a single track. [Up to that time two conductors had invariably been used.] It also seemed desirable that the metal rod bearing the train should also convey the current driving it. Lines such as I contemplated would not impede cultivation nor interfere with fencing. Ground need not be purchased for their erection. Mere way-leaves would be sufficient, as in the case of telegraphs. My ideas had reached this point in the spring of 1882, and I had devised some means for carrying them into effect when I read the account of the electrical railway exhibited by Professors Ayrton and Perry. In connection with this railway they had contrived means rendering the control of the vehicles independent of the action of the guard or driver; and this absolute block, as they called their system, seemed to me all that was required to enable me at once to carry out my idea of a continuous stream of light, evenly-spaced trains, with no drivers or guards. I saw, moreover, that the development of the system I had in view would be a severe tax on my time and energy; also that in Edinburgh I was not well placed for pushing such a scheme, and I had formed a high opinion of the value of the assistance which Professors Ayrton and Perry could give in designs and inventions.

Moved by these considerations, I wrote asking Professor Ayrton to co-operate in the development of my scheme, and suggesting that he should join with me in taking out my first Telpher patent. It has been found more convenient to keep our several patents distinct, but my letter ultimately led to the formation of the Telpherage Company (Limited), in which Professor Ayrton, Professor Perry, and I have equal interests. This company owns all our inventions in respect of electric locomotion, and the line shown in action to-day has been erected by this company on the estate of the chairman—Mr. Marlborough R. Pryor, of Weston. Since the summer of last year, and more especially since the formation of the company this spring, much time and thought has been spent in elaborating details. We are still far from the end of our work, and it is highly probable what has been done will change rapidly by a natural process of evolution. Nevertheless, the actual line now working does in all its main features accurately reproduce my first conception, and the general principles I have just laid down will, I think, remain true, however great the change in details may be.

The line at Weston consists of a series of posts, 60ft. apart, with two lines of rods or ropes, supported by crossheads on the posts. Each of these lines carries a train; one in fact is the up line, and the other the down line. Square steel rods, round steel rods, and steel wire ropes are all in course of trial. The round steel rod is my favorite road at present. The line is divided into sections of 120ft. or two spans, and each section is insulated from its neighbor. The rod or rope is at the posts supported by cast-iron saddles, curved in a vertical plane, so as to facilitate the passage of the wheels over the point of support. Each alternate section is insulated from the ground; all the insulated sections are in electrical connection with one another—so are all the uninsulated sections. The train is 120ft. long—the same length as that of a section. It consists of a series of seven buckets and a locomotive, evenly spaced with ash distance pieces—each bucket will convey, as a useful load, about 2½ cwts., and the bucket or skep, as it has come to be called, weighs, with its load, about 3 cwts. The locomotive also weighs about 3 cwts. The skeps hang below the line from one or from two V wheels, supported by arms which project out sideways so as to clear the supports at the posts; the motor or dynamo on

the locomotive is also below the line. It is supported on two broad flat wheels, and is driven by two horizontal gripping wheels; the connection of these with the motor is made by a new kind of fric tional gear which I have called nest gear, but which I cannot describe to-day. The motor on the locomotive will give as a maximum 1½ horse-power when so much is needed. A wire connects one pole of the motor with the leading wheel of the train, and a second wire connects the other pole with the trailing wheel; the other wheels are insulated from each other. Thus the train, wherever it stands, bridges a gap separating the insulated from the uninsulated section. The insulated sections are supplied with electricity from a dynamo driven by a stationary engine, and the current passing from the insulated section to the uninsulated section through the motor drives the locomotive. The actual line is quite short, and can only show two trains, one on the up and one on the down line; but with sufficient power at the station any number of trains could be driven in a continuous stream on each line. The appearance is that of a line of buckets running along a single telegraph wire of large size. A block system is devised and partly made, but is not yet erected. It differs from the earlier proposals in having no working parts on the line. This system of propulsion is called by us the Cross Over Parallel Arc. Other systems of supplying the currents, devised both by Professors Ayrton and Perry and myself, will be tried on lines now being erected; but that just described gives good results. The motors employed in the locomotives were invented by Messrs. Ayrton and Perry. They are believed to have the special advantage of giving a larger power for a given weight than any others. One weighing 99lb. gave 1½ h.-p. in some tests lately made. One weighing 36lb. gave 0.41 h.-p.

No scientific experiments have yet been made on the working of the line, and matters are not yet ripe for this—but we know that we can erect a cheap and simple permanent way, which will convey a useful load of say 15 cwts. on every alternate span of 120ft. This corresponds to 16½ tons per mile, which, running at five miles per hour, would convey 92½ tons of goods per hour. Thus, if we work for twenty hours, the line will convey 1850 tons of goods each way per diem, which seems a very fair performance for an inch rope. The arrangement of the line with only one rod instead of two rails diminishes friction very greatly. The carriages run as light as bicycles. The same peculiarity allows very sharp curves to be taken, but I am without experimental tests as yet of the limit in this respect. Further, we now know that we can insulate the line satisfactorily, even if very high potentials came to be employed. The grip of the locomotive is admirable and almost frictionless, the gear is silent and runs very easily. It is suited for the highest speeds, and this is very necessary, as the motors may, with advantage, run at 2,000 revolutions per minute.

The suspended rods are not suitable for high speeds. Centrifugal force would use great strains on them, and the vehicles would be shot up into the air at the points of support. Very high speeds might be attained for light trains with a stiff road, but we are for the present less interested in this application of our ideas. A smaller type of line with ⅝ rods and smaller spans is in course of construction.

This will probably soon be extended for a mile or so, now that we have gained some experience on the few spans of this heavier line.

At present we do not contemplate working lines extending more than five miles from a station, so that in a long continuous line we should require stations at intervals of ten miles. A single station could work, either simultaneously or in succession, a large number of lines radiating from it in many directions.

I cannot yet enter into questions of cost, and the company is hardly ready to undertake contracts, except perhaps for very simple cases. We have still to elaborate designs for sidings; for loading and marshalling the trains; and we have still to test the arrangements for governing and blocking. We have also to compare different systems of electric propulsion and blocking, and improve the design of many details in construction. All this will take time, but time and thought are all that is required. No new discovery is wanted; no unforeseen difficulty has been met with.

I am almost afraid to speak of the probable uses to which telpherage may be put. If I said all I thought, I should be told I was describing an electrical Utopia. The first and most obvious use of a telpher line is that to which existing wire tramways are already put—namely, the conveyance of minerals or ore from mines to canals, railways, or the sea. The suspended wire rope is specially suited for rocky uneven ground, and very heavy gradients could be worked. The telpher line has the following advantages over the present system: It can go round sharp curves, change the gradient as often as is desired, and be made of any length; any train can be stopped and shunted without stopping the others. If made with no working parts, as in the present example, the permanent way may be left idle for part of the year with no sensible deterioration.

Mineral traffic of this kind is, however, in my opinion, only one small part of the work which these lines can do. Where railways and canals do not exist, telpher lines will provide the cheapest mode of inland conveyance for all goods—such as corn, coal, root crops, herrings, salt, bricks, hides, and so forth—which can be conveniently subdivided into parcels of one, two or three hundredweight. In new colonies the lines will often be cheaper to make than roads, and will convey goods far more cheaply. In war they will give a ready means of sending supplies to the front. Moreover, wherever a telpher line exists, power is thereby laid on, and this power may be used for other purposes than locomotion—a flexible wire attached to the line will serve to drive a one, two, or three horse-engine, which may be used for any imaginable purpose, such as digging, mowing, threshing, or sawing. It is true that in the transmission of the power more than half may be wasted; but the proportion wasted is diminishing yearly, monthly, almost daily, with the growth of our electrical knowledge; and when we remember that by stationary engines in London power can be produced at the rate of about one halfpenny per hour for each effective horse, we shall not be alarmed at the prospect of doubling this cost when the power is delivered on a rough hillside, especially when we remember that the engine receiving that horse-power need weigh no more than one hundred pounds. Surely I am not too sanguine in expecting that great changes will be produced in agriculture by these new facilities for transport, coupled with the delivery of power at will from any point of the telpher road. It must not be supposed that I look on the new telpher lines as likely to compete with railways or injure their traffic. On the contrary, my feeling is that they will act as feeders of great value to the railways, extending into districts which could not support the cost even of the lightest railway. It is idle to endeavor to foretell the future of any new idea, but this much is certain—a novel mode of transport offering some exceptional advantages will be publicly shown on a practical scale to-day.

REGENERATIVE FURNACES AT THE MUNICH GASWORKS.

By Dr. SCHILLING and Dr. H. BUNTE.

Translated from "Journal fur Gasbeleuchtung" for Institution of Civil Engineers.

These furnaces have been described from time to time in their different phases of development, and the present description of them is consequent upon the adoption of a definite and complete form which is the result of scientific examination and practical experience during several years.

The peculiarity of these furnaces lies in the manner of working the generator, and in the systematic utilization of the waste heat of the oven in the regenerator. The generator is worked on the so-called "damp" system, i.e., the gasification of the coke is carried on by a mixture of air and steam. The advantages of this method have been made known by the experiments of Dr. Bunte on the capabilities of coke-generators, which were carried out at the Munich gas works, at the instigation of the German Association of Gas- and Water Engineers. These

advantages consist chiefly in the fact that the formation of clinker in the generator is prevented, whatever the kind of coke used; the earthy constituents remaining in the form of loose ashes in the grate, through which the air can easily pass. The experience of several years has shown that under this system the most various kinds of coke can be equally well used. When the generators have been fired first with coke from Saar coal, then with that from Bohemian coal, or with both together, mixed with that from Bohemian Plattel coal, there has not heen the least trouble in working them. It is just these kinds of coke that differ so much in their characteristics; for while the earthy constituents of Saar coal are easily melted, those of Bohemian coal are remarkably refractory. It is easy, simply by using a greater or less quantity of steam, to obtain the desired constitution of the ashes, and so prevent the formation of clinker. The porousness of the ashes, permitting the passage of air and steam, admits of an unusually uniform production of gas in the generator, and the furnace can be worked with only a slight draught. A stratum of ash several decimeters thick may be permitted to accumulate upon the grate without disturbing the regular working of the furnace, and the removal of the ashes need not be performed oftener than from every twenty-four to thirty-six hours. In order to obtain a uniform production of steam without loss of heat, the waste-gases, after leaving the regenerator, are made, before reaching the flue, to pass under and heat a water-tank situated below the fire-grate. The air and steam are mixed before entering the generator, and superheated by passing through channels adjacent to the flues conducting the waste-gases away. The tank containing the water for evaporation is covered, so that the steam cannot ascend direct through the fire-grate, but must pass out at the back of the tank, where it mixes with the air-supply, and the mixed air and steam, after passing through the canals above referred to, return to the fire grate, and enter the furnace above the covered tank. The generator-gases are introduced into the oven, a retort-setting, between the bottom retorts, where they mix with the heated air-supply for the secondary combustion. After heating the retorts, the products of combustion pass through flues in the regenerator situated below the oven. The air traverses channels situated alternately between the flues, the air always passing in a contrary direction to that of the waste-gases. Afterwards the gases heat the air and steam for the generator; and, lastly, they heat the water in the tank for the steam production.

The heating-surface of the tank is so designed that about 1.0 to 1.3 kilogrammes of steam will be produced per hour, which quantity is amply sufficient, under all circumstances, to prevent the formation of clinker. To regulate the quantity of steam, if required, a valve can be opened, which permits cold air to mix with the waste gases, and so reduce the temperature to the required degree. When it is required to remove the ashes from the generator, bars of flat iron are inserted through the sides of the generator, at a certain height above the grate, which support the superincumbent mass of fuel. The ashes are then withdrawn from a sealed door in the front. This operation is performed in ten minutes, and need not be repeated for thirty-six or even forty-eight hours. To replace the water evaporated in the tank, a funnel and siphon-tube are affixed to the side of the tank, into which a small stream of water continually flows, the surplus running off by an overflow. In starting a generator the waste gases are not allowed to pass through the regenerator at first, but are conducted direct to the chimney-flue, the change being made by dampers or valves after the oven has become thoroughly heated.

The working results taken from a bench of six ovens of this description are given on page 513.

The regenerators do not suffer deterioration, and require no repairs for years, as a four-years' experience with the older type of oven has proved; also the generator-shafts remain perfectly intact.

The following is an average of many analyses of the generator gases:

Carbonic acid	8.6	per cent.
" oxide	20.6	"
Hydrogen	15.0	"
Nitrogen	55.8	"
	100.0	

Gas produced per oven in	24 hours	=	2,300 c. m.	= 71,650 cubic ft. per ton.
Coals carbonized in retorts in	24 "	=	7,350 kilos.	= 7 tons 3 cwt.
Coke used for furnace in	24 "	=	800 "	= 15¾ cwt.
Gas produced per retort in	24 "	=	287 c. m.	= 10,135 cubic ft. per ton.
" " "	100 kilos. of coal	=	31 "	= 10,234 "
Coal carbonized per retort per diem		=	919 kilos.	= 18¼ cwt.
" charged per retort every 4 hours		=	153 "	= 334 lbs.
Coke used per 100 kilos. of coal		=	10.9 "	= 10.9 per cent.

The coke contained 14 per cent. of ash.

The generator possesses a temperature of 1,150° Centigrade on entering the oven, and the products of combustion leave it at 1,400°. After passing through the regenerator, and heating the secondary air-supply, they are reduced to 900°, and the air is heated to 1,000° or 1,100°. The waste-gases heat the air and steam for the generator to 350°, and they leave the water-tank and pass into the chimney-fire at a temperature of 550° Centigrade.

If the products passed direct from the oven to the chimney at a temperature of 1,400° the loss of heat would equal 64 per cent. of the total heating value of the coke; but by regeneration and the utilization of the heat to produce steam for the generator, this loss is reduced to 25 per cent. Of the total quantity of heat (= 39 per cent.) withdrawn from the waste-gases, 20 per cent. is communicated to the secondary air-supply, 6 per cent. to the primary air-supply, and 5 per cent. goes to the production of steam, while 8 per cent. is lost in radiation and conduction from the surface of the brickwork.

RELATIONS OF STATIC AND DYNAMIC ELECTRICITY.

By JOHN T. SPRAGUE.

From the "English Mechanic."*

Some time since I promised to explain a subject which has puzzled a good many, and as to which questions have often been put—viz., the often-quoted statement of Faraday as to the electricity contained in water and in a flash of lightning. That is the text of the present article.

Faraday said that the electricity contained in a grain of water is equal to that in a powerful flash of lightning: some quote him as saying it is equal to that of a severe thunderstorm. Now, we know from chemistry that the potential energy of a grain of water—that due to the combination of hydrogen and oxygen, which combination is broken up by electricity—is ft.-lb. 6,841÷9=760. It seems absurd, therefore, to compare this with the enormous forces exerted by lightning, seeing that it is hardly equal to raising the heat of a pound of water one degree Fahrenheit.

We must remember that when Faraday made this statement he was only beginning the study of electric quantities and relations, our present knowledge of which is largely due to his work. At that time nothing but vague generalities had been attained to, and little was known, or even imagined, as to the nature of electricity. Our present exact system of measurement was undreamed of, and Faraday was therefore unable to perceive that he was comparing together things having no relation such as he was working at; but it is strange that his statements should be constantly quoted as if they contained real truths, and that their true interpretation has not yet been clearly shown. Before attempting this it will be well to bring together the actual statements of Faraday and other workers on the same lines.

Faraday found that to decompose a grain of water required a current for three and three-quarter minutes, capable of keeping red hot a platinum wire $\frac{1}{104}$ of an inch in diameter. The very vagueness of such a measure at once shows the difficulties under which he labored. We can express this value definitely as a current, for the time given, of 3.13 ampères; or, better still, as a "quantity"

of 704.37 coulombs, the coulomb and ampère representing chemical action equal to .000158 grm. of hydrogen, or .00142 of water. He found that to effect the same decomposition by the machine required 800,000 charges of a Leyden battery of fifteen jars, each of 184 square inches of coated surface. Each of these charges, produced by 30 turns of a 50 in. plate machine, was capable of killing a rat, and was equivalent, chemically, to the action of a platinum and zinc wire, each of $\frac{1}{18}$ of an inch in diameter, dipping $\frac{5}{8}$ of an inch into 4 oz. of water containing one drop of acid, and developing current for about three seconds. Weber has also calculated that this charge from one grain of water, if placed on a cloud 1,000 meters (3,281 ft.) above the earth would exert an attractive force equal to 1,497 tons. So we have this enormous force and the potential slaughter of nearly a million of rats, originating in water, which we know is only possessed of less than 800 foot-pounds of energy, just about the amount of work a man ought to do in every five minutes of his day's labor.

De la Rive says: "Becquerel succeeded in finding that in order to decompose a gramme (15.43 grains) of water a quantity of electricity was required, equivalent to what would be furnished by 51,586,400 discharges of a battery having one square meter (1.9609 yds.) of surface, reduced to 20,063,456 when the charge of the battery is at its maximum. On reducing Faraday's result to the same conditions we find 21,850,451. The difference is very inconsiderable for experiments of this nature." This remark shows how very imperfect was the then existing knowledge. The value of the comparison depends wholly upon the potential given to the charge, not merely on the coated surfaces; and in the actual figures this shows a variation between 51 and 20 millions. He goes on to say: "Adopting the round number of 20,000,000 for one gramme, in order to decompose a milligramme (.0154 grain) of water, there are required 20,000 discharges of a battery of 1 square meter surface, or an electric pane having a surface of about 5 acres (Faraday's figures give 352 acres). Now, it is this same quantity of electricity that must be produced by the decomposition of this same milligramme of water by chemical means; and if it were accumulated so as to discharge itself instantaneously, it would be capable of producing the effects of a flash of lightning."

Weber established the relation between the statical and dynamical effects of the charge of a Leyden jar by measuring the first with the torsion balance, and the other by means of a galvanometer, to which the charge passed as a current by means of a long column of water which it traversed. His results were as follows:

"Through each section of a conductor, traversed by a current whose force is equal to the electro-magnetic unit, there passes, in a second of time, a quantity of electricity equal to $155{,}370 \times 10^6$ static units, an equal quantity of negative electricity traveling in the opposite direction.

"The quantity of electricity required to decompose 1 milligramme of water amounts to $106\frac{2}{3}$ times this quantity (this being the ratio of the electro-chemical to the electro-magnetic units) or $16{,}573 \times 10^9$ units of electricity of each kind; to decompose 9 milligrammes, or 1 equivalent, requires, of course, 9 times this quantity. This quantity of + electricity ($9 \times 16{,}573 \times 10^9$) accumulated on a cloud 1,000 meters above the surface of the earth, would exert an attractive force equal to 226,800 kilogrammes, or 208 tons."

It is further calculated that if all the atoms of hydrogen in one milligramme of water in the form of a column of one millimeter long were attached to a thread, and all the atoms of oxygen to another, then to effect the decomposition of the water in one second, the two threads would require to be drawn apart with a force of 147,380 kilogrammes, or 145 tons. The thread would need to be tolerably strong, seeing that this immeasurably exceeds the tensile strength of any known material; that is to say, this imaginary force, mathematically demonstrated, is greater than any cohesive force known to exist.

Now, let us translate this into modern expressions. We have seen that the dynamic value of one grain of water is 704.37 coulombs. What is the corresponding static expression which will replace the 800,000 charges and 325 acres of coated pane? We require a condenser

of capacity capable of receiving a charge of 704.37 coulombs, and the mere dimensions of this will depend upon the potential of the charge; starting with the unit 1 volt, and taking the capacity of the usual cable as 3.5 micro-farads per mile, we find that a force of 1 volt (say a Daniell cell) would require 2,465 million miles of cable to receive it, or speaking roughly, enough to wind 1,200 times round the earth and moon, or go twice round the orbit of the earth; that is to say, the earth would take two years in "paying" it out.

Let us put it another way: the (imaginary) capacity of the earth is 630 millions of electro-static units, or 700 micro-farads; this quantity (throwing in the odd 4.37 millions) would charge to 1 volt a million such worlds as ours! or, allowing the potential which Messrs. Ayrton and Perry calculated as necessary for their theory of the earth's magnetism—viz., 54 million volts, it would only require 54 grains of water to supply the required electricity, which, however, Prof. Rowland also calculated would result in bursting the earth to pieces.

Leaving all these fantastic unrealities in which some people delight, let us examine the common sense of the matter. Electricity is nothing but a word in which certain natural phenomena are embodied, and it involves two wholly distinct functions, both of which are commonly called "electricity;" for instance, people talk of the torrents of electricity poured forth in a flash of lightning, and the torrents of electricity streaming out of a dynamo-machine, and the idea of "quantity" in these two is wholly different; each belongs to one of the two functions which together constitute electricity. Those two functions are: (1) The molecular construction of *matter*—that to which the idea of the "grain of water" is collated, and which we may symbolize as Q, for quantity; (2) The stress put upon those molecules, the "potential" symbolized by E for electro-motive force. Static electricity has no existence as a "quantity;" it is simply the evidence of the stress existing in that class of matter which is called di-electric, or which resists the passage of electric current. All the ideas we have been considering are based upon the impossible isolation of Q; the real phenomena always depend upon $Q \times E$. Matter and energy together constitute electricity.

Now in the case of water the E is a potential of volt 1.5, and what we have to take into account in the case of the grain of water is, therefore, $704 \times 1.5 = 1,056$, which multiplied by .7373 gives us a little over the 760ft.-pounds, which we know is the total energy of the grain of water. Let us now consider the case of the lightning flash, the Q of which is this same 704 coulombs, or the grain of water. The EMF of the lightning flash is very uncertain; it is calculated, from experiments with De la Rue's 14,000-cell battery, that the EMF of a flash one mile in length is 3,604,000 volts, and this by the same process gives us 1,872 millions of foot-pounds, and enables us to realize why this "grain of water" can produce such great effects when concentrating this amount of energy in some point of high resistance which appropriates great part of it as "work" in possibly one millionth of a second.

But the *current* of such a flash is not, as some say, a very small one, because current is $Q \div T$, related to time, and the ampere means 1 coulomb per second. We have here, then, 704 coulombs, passing in a time variously estimated as one 20,000th to 1 millionth of a second; taking the lowest estimate, we have a current of 14 million amperes, greater than all the dynamos on earth could generate. From this it is pretty clear that no lightning-conductor is ever called upon to carry such a current; either it lowers the potential so gradually that the actual flash when it occurs is reduced to a very small one, or, on the other hand, it only carries a small part of the energy of the discharge, which is largely distributed over surrounding space.

It should be clearly realized that the distinction of static and dynamic electricity is artificial and misleading as usually conceived. *Frictional* electricity differs from that of the battery only in being produced in a circuit of highly resisting substances, and therefore under high potential. But so-called *static* electricity has no existence at all; it is a name which includes either *stresses* set up in dielectrics—that is, actions of energy in an *inductive circuit*, or else of very small currents in a *conductive* circuit of great resistance,

all dielectrics having some small conductive capacity.

The static phenomena are exactly parallel with the dynamic. It may be said that there is this difference: that an electric current requires a continual expenditure of energy to maintain it, while the static charge, once produced, may be (theoretically) maintained indefinitely without the expenditure of energy. The theory which says so is a mathematical one, commencing with the favorite formula, "Let us assume." That a charge can be so maintained is an assumption contradicted by every known fact. The fact that no such charge has ever been produced is explained, in order to fit the theory, by the invention of "leakage"; that is to say, there exists no such perfect dielectric as the theory requires for its foundation; all dielectrics are partial conductors, and this means *current*. But even if we imagine such a dielectric, it would involve, not static *electricity*, but energy, stored "potential," as strains upon the molecules of the dielectric; just as we have in the second battery, not electricity, but energy, stored potential as chemical forces. Further, no such charge could prove its existence as a charge.

When it moves an indicator of any kind, that involves energy expended and current passing. The simple fact is, that the current is infinitesimal and the energy inappreciable. But we have just such currents in dynamic electricity. Experiments have been made as to deposit of minerals, in which the resistance of the depositing cell is so great, that the current would need a century to deposit one grain of metal, and yet that current can indicate itself upon a galvanometer.

In such experiments the depositing cell is a static field, a condenser, in which only a small force exists. Its electrodes indicate a static charge, exactly as do the static conductors, and the electrolytic cell and static condenser are exact analogues of each other.

I think these relations are more clearly shown in a tabular form, which contrasts the two circuits and principles, than in any other manner, and therefore I append such a table, which I have prepared for my book.

Static	ELECTRICITY	*Dynamic*
Energy *potential*: stored in a field of force as stress in a dielectric.	means	Energy *kinetic*: acting in a conducting path, along molecular chains.
Inductive	CAPACITY	*Conductive*
Area of dielectric.	varies as	Area of conductor.
Spec. ind. capacity of dielectric.	"	Specific conductivity of material.
Thickness	inversely as	Length.
Units of Charge stored under unit Potential.	is measured by	Units of current transmitted under unit EMF.
The Potential in volts required for unit charge.	or by	The EMF in volts required for unit current.
Inductive	RESISTANCE	*Conductive*
Reciprocal of inductive capacity.	is the	Reciprocal of conducting capacity.
CHARGE	ELECTRIC	CURRENT
Potential, in volts.	is as	Electromotive force, in volts.
Resistance	inversely as	Resistance
Lines of force under stress.	is related to and dependent on	Lines of equivalent molecules forming chains.
Inversely as the several resistances.	divides among several circuits	Inversely as the several resistances.
ENERGY, OR WORK.		
Square of charge in unit or equal resistance.	is as	Square of current in unit or equal resistance.

FORMULÆ.

Charge	$\frac{E}{R}=Q$	Current	$\frac{E}{R}=C$
Potential	$Q\times R=E$	Electromotive force	$C\times R=E$
Resistance	$\frac{E}{Q}=R$	Resistance	$\frac{E}{C}=R$

REPORTS OF ENGINEERING SOCIETIES.

AMERICAN SOCIETY OF CIVIL ENGINEERS.—NOVEMBER 7th, 1883.—A paper by Mr. Edwin Thacher, M. Am. Soc. C. E., was, in the absence of the author, read by the Secretary, the title being: "Description of a Combined Triangular and Suspension Bridge Truss, and Comparison of its Cost with that of the Warren, Pratt, Whipple, and Howe Trusses." The writer presented drawings and descriptions of a truss formed by a combination of the triangular and suspension systems, the primary system being composed of top and bottom chords, and a web of struts and ties arranged in the form of triangles, free to change figure from the effects of temperature. The center ties extend each over not less than two panels, and over not more than the number in half span, less one. A careful analysis was presented of the strains in these bridges; the paper also having tables giving the uniform live loads, which may be substituted for the wheel loads, of the leading types of 10-wheel and consolidation engines, in spans ranging from 10 to 500 feet. The writer discussed the defects of various forms of bridge truss, com-

pared with what were considered the advantages of the particular truss described in the paper. He also presented estimates of the cost of bridges built upon this principle as compared with the cost of the Warren, Pratt, Whipple and Howe trusses, deducing a lower result for all spans built by this method. He presented comparative estimates as to the economy of bridges built entirely of iron as compared with those built with the combination of wood and iron, with the result that the ultimate cost of the combination bridge was considerably lower than the other.

Several bridges built upon this plan, at various points in the United States, were described.

ENGINEERS' CLUB OF PHILADELPHIA—REGULAR MEETING, OCTOBER 6th.—Mr. Edward Thiange presented an illustrated description of a method of earthwork computation by means of diagrams, constructed from the proposition: "The areas of similar figures are to each other as the squares of their homologous sides." An idea may be had of their nature and uses by the following directions: To get the average volume in cubic yards of a station (in embankment), to the center fill at each end add the constant height of the "grade triangle" (which is formed by the road bed and the side slopes produced); at the resultant heights on the diagram, measure, with the scale of cubic yards, the lengths of the ordinates terminated by the slope lines at each station respectively; their sum, diminished by the "grade prism," is the average quantity for the station of 100 feet.

A paper upon "Economy in Highway Bridges," by Prof. J. A. L. Waddell, was read. Its objects are to determine the most economical depth and number of panels for spans from 40 to 200 feet; the lengths at which it is better to change from pony truss to thorough bridge, and from single to double intersection; the exact dead loads, and the amounts of lumber and iron for each case.

The Secretary presented a photograph and description of the Lift Bridge for double track railway over Oswego Canal, N. Y. W. S. & B. R. R., kindly contributed to the Club by Mr. Jno. A. Partridge (Member Am. S. C. E.), Div. Engr. Length of truss, 94 feet; height, 23 feet; width, 30 feet 4 inches; angle with center line, 38°; weight of bridge with machinery, 146 tons; weight of counter-weights, 138 tons; height of lift from bridge seat, 10½ feet; time required for lift, 30 seconds.

REGULAR MEETING, NOVEMBER 3d.—"Notes upon Roads, Streets and Pavements," by Mr. Chas. H. Haswell, were presented. Classification, grade and construction are first treated, and then special notes made with regard to Macadamized, Telford, gravel or earth, corduroy and plank roads, and asphalt, wood, block stone, rubble and concrete pavements. Miscellaneous notes upon road making, complete a very compact collection of data.

Mr. E. A. Geiseler read an illustrated paper upon "Tides and Newton's Theory of Them."

Mr. Allen J. Fuller presented notes upon the "Effect of Frost upon Fire-plug Casings." He referred to a general impression that the freezing of the earth around fire hydrants has a tendency to grip fast to the frost jacket, and lift it with the expanding or heaving earth, which he denied for the following reasons:

1st. The frozen earth slides on the surface of the frost jacket, because its expansion is greater than that of iron.

2d. As the expansion of the earth must be in proportion to the intensity of the cold, so will it be greater above than below a given point, therefore, the first foot of frozen ground will have a greater upward movement than that which is below it, and the second foot greater than the third, etc. Thus it will be seen that the earth below a given point rises more slowly than that above, and its friction is opposed to the one above.

3d. If this is true of feet it is true of inches, and of portions of an inch; therefore, there is a retardation movement throughout.

4th. The upward movement of the ground, the freezing being greatest towards the surface, and such movement involving a more complete fracture of the earth surrounding the frost jacket, it follows that the friction is less at this point than that below it, and in consequence there is less power to move upward than downward.

Of course, the above does not appiy to any construction that the frost can get beneath.

Mr. Frederic Graff noted and described the form ot wooden casing which had been successfully used in the early practice of the Philadelphia Water Department.

In response to the theory advanced in regard to the action of frost in raising the casings of fire-plugs, and to the statement that if the base of a structure extended below the frost line it would not be lifted, Prof. Haupt remarked that he thought the theory was in part sustained by the fact observed by some of the district surveyors, and verified by the accurate measurements they were obliged to make, that fences moved bodily to the South and East in consequence of the action of the sun and frost upon the ground on opposite sides of them. He thought, also, that the deductions concerning the immobility of structures resting below the frost line was not fully sustained by the facts, as, in the Northwest where ice forms rapidly, he had heard of numerous instances of piles, driven for bridges and extending some distance below the frost line, having been raised as much as five to six inches in a single night, and he conceived the action in this case to be similar in kind to that of piles driven entirely through solid ground, the only difference being in the amount of the resistance offered by friction and weight of pile. The water in freezing around the pile acts upon it as a gripper or vise, and the expansion of the various strata or laminæ of water as they become converted into ice act as levers to force up the pile.

The Secretary did not consider the case cited by Prof. Haupt as parallel, as the so-called piles being driven through water and soft mud, were probably columns resting upon their

bases and depending but little upon the frictional resistance of the material through which they passed. Therefore, the expansive force upward of the freezing water would be opposed by little more than the weight of the pile, whereas in a fire hydrant casing or other deeply planted post, the presumably well-rammed material around the whole length under ground would offer such proportional frictional resistance, as to cause the freezing earth to slide up the post rather than to lift it. If the ice could be supposed to act downwards upon the piles in question it is hardly likely that it would have forced them further home.

Prof. Haupt also exhibited a "History of the Manual Arts, or the Inventions of Human Wit," published by Mr. Herringman, London, 1661, and read extracts therefrom describing the clepsydrae or ancient water clocks.

The Secretary read the following account from the *Mexican National* of Laredo, Texas, of a bridge construction by Mr. C. A. Merriam, Member of the Club, Asst. Supt. and Engr. F. Div. M. N. Ry. "On the 6th day of September (the anniversary of loss of bridge last year) the Mexican National Railroad bridge was carried away by high water. On Monday the 16th the first pile was driven for the new structure, which was completed on the 23d ult., and trains are now running regularly. This is pretty quick work, the erection of a bridge 600 feet long in seven days."

The Secretary narrated his experience on behalf of the Club, and read extracts from correspondence, etc., with the Custom House, through the stupendous inscrutability of the management of which the Transactions of the Society of Engineers of London, and of the Institution of C. E.'s of Ireland, are charged with duty, and all the other foreign societies upon the exchange list of the club are admitted free.

ENGINEERING NOTES.

THE LOWER THAMES VALLEY SEWAGE.—We last week referred to the circumstance that there was at lengh a probability of the question of the disposal of the sewage of the Lower Thames Valley being solved by a scheme propounded by Messrs. Mansergh and Melliss, C. E., of Westminster. At that time the question of site was under the consideration of the local authorities, three places having been referred to in Messrs. Mansergh & Melliss' report as appropriate for collecting and dealing with the sewage. These were, respectively, Barnes, Mortlake, and Ham Fields, and the Mortlake site has now been selected for that purpose. We now append particulars of the method of utilization, which it is proposed to adopt, and which method is already in operation at Chiswick, on the opposite side of the river, with satisfactory results. This being so, and as the details of the works have not yet been decided upon, it will make the matter clearer if we describe the existing works at Chiswick, the principle remaining the same whatever the locality of the works. The sewage is brought by gravitation to the pump well in the sewage works, and is then pumped into tanks, the total lift being 19 feet. The chemicals used are crude sulphate of alumina and chalk lime, to which is added a little yellow clay. The milk of lime and the alumina and clay are mixed together by machinery and run into the pump well, where the action of the pumps secures a complete incorporation of the chemicals with the crude sewage. The sewage in the well has a dark brown tint, and is without any smell, as the rapidity with which it is delivered to the well prevents decomposition setting in. The whole of the solid portions of the sewage are broken in their passage down to the sewers, so that they reach the works in solution. A large strainer holds back the solid matter not in solution, and which might damage the pumps on its passage to the tank. The engines are in duplicate, so that in case of accident or during repair the work may be proceeded with. The sewage, mixed with the chemicals, passes into the distributing channel, from which it is run into any one or any number of the eight tanks which receive the sewage. Four of these tanks hold 50,000 gallons each, and four 150,000 gallons each. When a tank is filled it is allowed to rest, and precipitation immediately commences. In about three hours the process is completed. At one end of the tanks a floating valve, which has its mouth one inch below the level of the water and which rises and falls with it, is placed, by means of which the surface water is drawn off without disturbing the precipitated matters at the bottom. When this valve is opened the clarified water rushes into a lower channel, passes through a large filter bed filled with coke, which is afterwards used as fuel, into a third channel, and then by way of the effluent pipe, into the river. The water is now clear, bright, and inodorous. It is only claimed for this process that it purifies the sewage so that the effluent water can be turned into the river without causing any nuisance or detriment. It is not, however, claimed for it that it rescues all the valuable manurial properties from the sewage, as the free ammonia is not precipitated by it. The bottom of the tank from which the top water has been run off is covered with a thick, black sludge, which, when treated immediately, does not give off any offensive smell. The tank bottom inclines towards the engine-room, and to this end the sludge gravitates. From here it is lifted by a pump into an underground carrier and deposited in earth tanks, wherein the water mixed with it evaporates and percolates away until the sludge becomes portable. It is then given away to market gardeners and others, as the local authority have not hitherto sought to realize any profit from it, and is applied to land with good effect.

THE HYDROMOTOR.—We have a report of a trial which took place on October 11th, on the river Elbe, near Dresden, of a vessel, the name of which, "Hydromotor," explains the principle of propulsion. The writer of the report states that the invention "promises to effect an important change in the propulsion of vessels of all classes." He adds: "The inventor, Dr. Emil Fleischer, has applied the principle of hydraulic reaction to the propulsion of ships in a manner which, according to the testi-

mony of officers of the German Navy, is as simple as it is effective. It does completely away with all risks from accidents to rudder or machinery, for there need be no rudder, and there is scarcely any machinery; solves the problem of avoiding loss of power through transmission, for there is hardly any transmission; and, finally, reduces the perils of the sea to a minimum, so far as injuries to the machinery are concerned. For, the same immense force that propels the vessel can, at a moment's notice, be used to pump her so effectively that she would remain afloat with a considerable leak in her bottom; while in case of fire there would be no diffiulty in quenching it." The writer then gives an account of the trial of the vessel, which is 197 feet long by 23 feet beam, and draws 2 feet of water and is constructed of iron. "The navigation of the Elbe near Dresden," he says, "presents many obstacles in the shallows and rapids which occur at frequent intervals. Noiselessly, and without any oscillation, did the large vessel—large as compared with the steam-craft plying on that part of the river—after the simple turning of a lever by the captain on the bridge, commence its trial trip, stemming the current, and keeping an even course under the right bank of the river. The only noise audible was that of the rushing of the water from the tubes fixed a little above the level of the river, and nearly amidships, on both sides of the vessel. Another turn of the lever and the action was reversed. The vessel comes to a dead stop in less than her own length. By the alternate use of the levers she may be turned around on an almost stationary pivot. The captain handles the levers on his bridge independent of all communication with the engine-room." We are finally told that arrangements are now being made to apply the invention on a very large scale. Lacking a detailed description of the vessel tried in Germany, it is impossible to pronounce an opinion upon its merits; but, from what has appeared, the "invention" seems to be a resuscitation of the principle first applied on a large scale by Ruthven, over seventeen years ago, to the Nautilus, and later to the well-known Waterwitch, and one or two other vessels. Mr. Ruthven worked out the principle as early as 1839, and applied it to a small vessel 9 feet long, and later on, in 1844, to a larger one, 40 feet in length. Notwithstanding all his efforts, however, to induce shipowners to adopt his mode of propulsion, he failed, for the simple reason that, as compared with screw propulsion, power for power, the hydraulic propeller lagged behind the screw by about half a knot per hour. Unless Dr. Fleischer, therefore, has succeeded in meeting that objection, he will taste of the fruits of failure and disappointment just as Ruthven, and many an enthusiastic inventor before him, did.

IRON AND STEEL NOTES.

THE POSITION OF THE STEEL RAIL TRADE.—The outlook in the immediate future before steel rail manufacturers is grave in the extreme. Prices which have fallen almost uninterruptedly since the beginning of 1882, are now at a lower figure than they have touched since the midsummer of 1879. According to report, one of our southern railways has given out within the last fortnight a contract for about 4,000 tons of steel rails at a price under £5 per ton delivered in the Thames; and it is difficult to see how the makers can realize from this more than £4 10s. per ton at their works, if that amount will be left to them after paying freight and other charges. This is bad enough, but it is by no means such a desperate state of matters as was described in the sensational story to which a contemporary gave prominence about two months since. It shows, however, that prices are still tending downwards and we are afraid that they have not yet reached the bottom. So far as can be judged at the moment, balancing the probable demands of consumption against the power of production, the next year or two will witness a sharp struggle for existence amongst steel rail makers. Persons well versed in the iron trade look upon the steel rail branch of it as the one before which the most gloomy prospect lies. It behoves manufacturers, therefore, to study the situation carefully, and not, ostrich-like, to blind themselves to the facts that surround them, and drive on in a happy-go-lucky fashion, trusting that things will improve.

The cause of the existing state of things ought to be well known to every one connected with the iron trade, yet it would appear from the policy pursued by many manufacturers that they are oblivious to it. Over-production lies at the root of the evil. During the nine months ending September 30 this country exported, according to the Board of Trade statistics, 579,-421 tons of steel rails, as against 552,555 tons during the corresponding period of last year. In the face of these figures it cannot be said that the demand has fallen off, for the quantity going into home consumption, from all appearances, will be very much the same in both years. This increase in the quantity of steel rails shipped is the more remarkable when the falling off which has taken place in our total iron exports is remembered. Given a growing consumption, accompanied by receding prices, and there is nothing left but to come to the conclusion that the power of production has been increased too rapidly; otherwise, manufacturers must not object to the charge of over-production, so long as they continue to complain of the low level to which prices have come. The enormous strides which have been made of late years in the production of Bessemer steel form one of the most remarkable features of modern industrial progress. The output has been more than quadrupled during the last decade, for, from 410,000 tons in 1872 the production has risen by leaps and bounds to 1,673,649 tons in 1882, and the increase since 1880 alone has been more than 50 per cent. That this astonishing rate of expansion can be maintained appears incredible, but we believe that the production this year will prove to have been still larger than it was in 1882. At the end of last year ten converters were in course of construction, some of which have come into operation already; while the policy of existing works, down to the present, has been to increase the output as far

as possible, so as to make up for a diminishing margin of profit by a larger turnover. But supposing that last year's production is simply maintained this year, and leaving the question of any increase on the one side, let us see what it means. Of course the whole of the Bessemer steel made is not turned into rails, but the greater proportion of it is. In 1882, out of the 1,673, 649 tons of ingots produced, about 1,325,000 tons were rolled into rails, of which the output that year was 1,235,785 tons. This year the proportion which the make of steel rails will bear to the production of ingots is likely to be considerably greater, because the export demand for blooms having almost entirely ceased, the manfacturer will have more steel to roll into rails, unless an outlet can be found for the metal in some other form. It will be sufficient, however, to base our examination upon the production of last year. Taking as an average section of rail one weighing 56 lb. per yard, we find that the production of this country in 1882 was sufficient to cover 14,044 miles of single track, that is to say, that in 18 years, without any further increase in its power of making steel rails, Great Britain alone could double the existing railways possessed by the world. In less than two years it could relay the lines in operation in India, Canada, and the whole of our Australian colonies, and within another year could equip all the railways of the United Kingdom. But it is not only in this country that the manufacture of steel has been brought to such a pitch; the production in the United States has been even more remarkable, the quantity turned out by the rolling mills of that country in 1882 having been 50,000 tons more than our own, while on the Continent rapid advances have been made likewise. In 1882 the world's production of Bessemer steel is estimated to have exceeded 5,000,000 tons, and from the information we can obtain the quantity of steel rails rolled was probably not less than 3,750,000 tons. Some idea of the meaning of this enormous quantity will be gained when we state that it is sufficient to relay in six years all the railways built in the world. The capability of the world to continue absorbing rails at this rate may well be doubted. Of course the total mileage of railways already in existence is comparatively trifling, being estimated about 265,000 miles, and there is still a vast field for its extension—Asia, Africa, and South America being as yet but very sparsely furnished with iron roads—but in the very nature of things, the growth must be gradual. It is seldom that the construction of a line of railway is undertaken until the necessity for it has been demonstrated, and it has become a matter of importance to join two centers of trade or to open up a rich district. It is only in America that they build a road running from nowhere to nowhere. Moreover, the making of a railway is a costly undertaking, and even where governments take the work in hand the sinews of war are the chief consideration. Hitherto the building of railways has proceeded by fits and starts, and after every period of activity there has been a time of reaction and comparative cessation of work. It is only the operation of the natural law that all exertion must be followed by repose. Such a period of activity has been experienced during the last three years, and if the unprecedented rate of railway construction which has prevailed throughout the years 1881–3 has not been sufficient to raise the price of steel rails even to a moderate level, what is to be expected when stagnation sets in? As we have seen, the power of production of the works in this country alone is already more than 50 per cent. greater than it was previous to the burst of activity in railroad making. Any further increase is nothing short of suicidal.

But to every cloud there is a silver lining. In considering the prospects we have confined our remarks so far to the immediate future, say the next three or four years. Those works that can manage to survive the struggle will, in all probability, find plenty of work to do. The low range of prices which may be said to be inevitable will stimulate consumption, and as our colonies and dependencies increase in population and wealth the present mileage of railways will not satisfy their wants. In fact the existing mileage is as nothing to what well-populated countries can maintain. The number of miles of railroad open in the United States is about 116,000. Messrs. Poor, in their *Manual* for 1883, say: "Included in the available area of the United States are 3,000,000 square miles. A ratio of one mile of railroad to ten square miles of area will give 300,000 miles of line. Construction will proceed uninterruptedly until such an extent of mileage is reached." The President of the Iron and Steel Institute, in addressing the May meeting of that body, remarked that India, with a population of 250,000,000 has less than 10,000 miles of railway, whilst the United States, with only 50,000, possesses more than 100,000 miles. Such figures show the scope there is for new railways, but their construction is a question of time.—*Iron.*

RAILWAY NOTES.

From data just published it appears that the number of locomotives running on German railways at the end of 1881 was 10,869, only 530 of which were supplied by foreign manufacturers. Borsig, of Berlin, supplied 2,345 engines; next came the Maffei Company, with 966; Hannoversche Locomotivfabrik, 939; Henschel and Sohn, 871; Sachische Maschinenfabrik, 768; the Vulcan Company, Stettin, 755; the Karlsruher Maschinenbau-Gesellschaft, 638; the Esslingen Company, 634; Wohlert, 629; Schwartzkopff, 574; Krauss, 278. The above firms represent the principal German locomotive builders. The railway companies constructed in their own shops only 241 engines. Of the locomotives imported, 286 came from Austria, 193 from England, and 51 were built in Belgium.

The directors of the Midland Railway Company has just completed the purchase of all the Pullman drawing-room cars running on its line from London to Liverpool, Manchester, Glasgow, and Edinburgh. It is understood that the Midland Company takes possession on the first of November, and the special car conductors have all had notice to leave the service

of the Pullman Car Company. The Midland intends to utilize these cars as first-class carriages without any extra charge. It is expected that all first-class passengers will travel in the cars, and that this will enable a number of first-class carriages which at present run half empty to be tsken off the trains. Each Pullman car weighs about 21 tons, and as two are attached to the Scotch express and the 5 p. m. express from London to Manchester and Liverpool, a great loss is incurred in drawing them when they are frequently nearly empty. The change will secure a reduction in the dead weight of the trains, and the use of two engines on many trains will be avoided. As a second engine costs about 1s. extra per mile run, there will be a large saving in working expenses. The Pullman Car Company still retains the sleeping cars.

ORDNANCE AND NAVAL.

THE change that is taking place in the tonnage of the vessels that do the carrying over sea is very well exemplified by a statement of the tonnage that left the port of Sunderland during the first half of the present year. In that period the average tonnage of the vessels frequenting the port had increased by about 14 per cent. In the number of vessels there is a considerable variation. Comparing the half year with the first half of the past year, it is seen that of vessels frequenting the port of less than 150 tons register there has been a decrease of 224, and of the vessels between 150 and 250 tons, the decrease is 53 in number; but all above that show an increase. Between 250 and 350 tons the increase is 36; between 350 tons and 500 tons it is 4; between 500 and 750 tons it is 104; between 750 tons and 1,000, it is 46; and above 1,000 tons the increase is 29. It is thus evident that small vessels are less frequenting the great port of North Durham, and that the work is done by fewer and larger vessels; and this experience will probably be found to be general, for the tendency is marked towards the use of steam-propelled vessels, whilst the small sailing craft that a few years ago were the chief support of the ports are dying out. The result is that there is a tendency towards the use of the ports that can offer the fullest facilities for vessels of large tonnage, and as Sunderland has very considerably increased its facilities of late, it finds the benefit of the change, and some of the smaller ports lose. And probably that change will go on, for it is not likely that the small vessels that now exist will be renewed when they are removed from the register; so that the ports that prepare themselves with the fullest facilities for the loading of large vessels will reap the benefit.

THE 48-TON GUN.—The most important trial of naval gunnery which has ever taken place off the port of Plymouth took place recently, when the *Agamemnon*, double screw turret-ship, was taken into the offing for experiments with her four 38-ton guns. The *Agamemnon*, which was built at Chatham from designs of Mr. Nathaniel Barnaby, may fairly be regarded as a small *Inflexible*. Her style is the same, and her fittings are identical. The objects of the trials were not so much to test the accuracy of the gunners as to indicate the resisting power of the ship, when it became necessary to discharge any or all of the powerful weapons with which she is armed. On leaving Plymouth Sound, a course was at once set for the eastward of the Eddystone, where it was intended to commence experiments. It was 10.30 before the guns of the fore turret commenced to fire, and the first two rounds were of a very unimportant character. Scaling charges of blank powder had been introduced in order to test the efficiency of the guns. These were fired in quick succession, and then commenced the real work of the day—the firing of the right and left gun on the port beam horizontally directed with a half charge of 105 lb. The effect on the ship by the firing of these guns was in no way felt. Every precaution had been observed in connection with the experiments, and inasmuch as fire might have been left in the guns, owing to their being "chambered," it was resolved to inject water into them after they had been searched by means of the sponge. In this way there was no fear of fire remaining and igniting the charge which might be soon after placed in the piece of artillery. The guns in the fore turret were then fired in rapid succession with full and battering charges, the former representing the discharge of 157 lb. of powder with 800 lb. of shot, and the latter 210 lb. of powder with similar projectiles. The guns were also fired simultaneously with battering charges. Similar experiments were then made with the guns in the after turret, and some delay here occurred by the giving out of one of the packing presses, which was struck with great force by the recoil of the gun from the full charge. The most severe test of the day took place at about 4.30, when all four guns were fired simultaneously. The guns were charged with 840 lb. of powder and 3,200 lb. weight of shot. The effect of the combined explosion on shore has been reported to have been very great. On board, however, but a slight concussion was felt. As the result of the day's experiments, the *Agamemnon* is regarded as one of the most powerful additions to the British fleet. With the minor discharges of cannon the greatest damage done was the destruction of glass and the carrying away of boats' covering. When the combined firing of the four 38-ton guns took place, the temporary bulkhead underneath the superstructure was driven out and the angle irons were twisted. This bulkhead had only been erected at Devonport as a protection to the boilers for the new engines put into that ship to drive the hydraulic machinery. The weakness was at once detected, and will be remedied. The automatic sights and the loading and elevating gear worked in the most successful manner.

THE NEW RUSSIAN ARTILLERY.—The *Moskoya Sbornik* publishes particulars of the new artillery adopted for the Russian Navy, from which we take the following information. Prolonged and careful experiments have been made during a considerable time, for the pur-

pose of ascertaining what weapons, powder, and projectiles could with most advantage be substituted for the clumsy and inefficacious material hitherto in use. The experiments of the Krupp firm were taken as a basis, and the result of several trials has been the adoption of models upon which the new guns are being manufactured at the Obukow foundry. In the four-pounders (9 cm.) and eleven-pounders (11 cm.), the rifling has an increasing twist, and its angular inclination divides it into three sections; at the commencement it is rectilinear, it next follows an arc of a circle, and afterwards becomes again rectilinear, both rectilinear portions being tangential to the arc. The charge is fired from above, and the vent is inclined at an angle of 41 deg. to the axis of the bore. The following Table gives the principal details:

Items.	4-pounder. (9 cm.)		11-pounder. (11 cm.)	
	ft.	in.	ft.	in.
Total length of gun.	6	10	6	11
Length of rifled tube.....	5	2.25	5	1.2
Length of chamber.	0	10.75	0	10.5
Weight of cannon without breech-block.....	cwt. 8	lb. 100.5	cwt. 12	lb. 29.5
Weight of breech-block.....	0	103.5	1	40
Weight of projecile.	0	16.75	0	30.5
Weight of charge...	0	3.1	0	5
	ft.	in.	ft.	in.
Initial velocity.....	1476	10	1355	0
Number of grooves.	24		24	

The new 9 inch guns have not yet been definitely determined on. The 28 cm. guns of the new type have also been constructed at Obukow, and are to be used for arming some of the ships of the fleet, especially the gun-boat Tuscha. In the comparative trials of accuracy in shooting between the old and new guns, the deviation of the former was vertically 5 ft. 3.38 in., and horizontally 5 ft. 5 in.; while that of the latter was only 12.6 in., and 3 ft. 2.58 in., respectively. The following are the principal dimensions, &c.:

	28 cm. canon.		
	ft.	in.	
Total length of gun.....	19	10	
Length of rifled tube.....	13	0.5	
" " chamber.....	4	0.75	
Number of grooves.....	64		
	tns.	cwt.	lb.
Weight of gun with breech-block	30	19	14
" " projectile.....	0	4	72.25
	0	5	48.5
" " charge.....	0	1	33.5
	ft.		in.
Initial velocity.....	1,640		5
	1,312		4.25

The experiments with this gun and others of still larger calibre proved the necessity of deciding upon the species of prismatic powder most suitable for heavy charges with large projectiles, *i.e.*, upon the density and dimensions of the grains. The powder at present in use with heavy projectiles has a density of 175, and produces so great a pressure against the sides of the barrel that it is considered impossible to increase the charge further with a view to obtain the desired initial velocity. Experiments are therefore still being carried on, both by the Messrs. Krupp and the Russian powder manufacturers, and some more satisfactory results have already been obtained, though no final decision has yet been arrived at.—*Engineering.*

BOOK NOTICES

PUBLICATIONS RECEIVED.

ANNUAL Report of the Chief of Engineers of the United States Army.

Report of the United States Commissioner of Education.

To Mr. James Forrest, Secretary of the Institution of Civil Engineers, we are indebted for the following papers:

Continuous Girder Bridges. By Thomas Claxton Fidler, M. Inst. C. E.

Water Supply and Irrigation of Canterbury Plains, N. Y. By George Frederick Ritso, A. M. I. C. E.

Blasting a Channel in the River Yarra. By Joseph Brady, M. Inst. C. E.

The Preservation of Iron. By Benjamin Howarth Thwaite. A. M. Inst. C. E.

A Deep Boring at Northampton. By Henry John Eunson, Student Inst. C. E.

Treatment of Complex Ores. By James Warne Chenhell, A. M. Inst. C. E.

Raising the Steamship Austral. By John Standfield, M. Inst. C. E.

Graphic Methods of Computing Stresses. by Charles Ormsby Burge, M. Inst. C. E.

The Waterworks of Edinburgh, Port Elizabeth, S. A., and Peterborough. By Alexander Leslie, M. Inst. C. E.; John George Gamble, M. Inst. C. E., and John Addy, M. Inst. C. E.

Signal Service Notes. Prepared under direction of Bvt. Maj.-Gen. W. B. Hazen.

No. 1. Report of the Michigan Forest Fires. By Wm. O. Bailey.

No. 2. The Use of Homing Pigeons for Military Purposes. By Wm. E. Birkhimer.

No. 3. To Foretell Frost. By James Allen.

No. 4. Use of the Spectroscope in Meteorological Observations. By Winslow Upton, A. M.

No. 5. Work of the Signal Service in Arctic Regions. By Bvt. Maj.-Gen. W. B. Hazen.

MONCKTON'S PRACTICAL GEOMETRY. By JAMES H. MONCKTON. New York: William T. Comstock.

This treatise is not unlike several others which have been constructed for the same general object.

The learner is afforded a series of lessons beginning with the simplest problems in geometry, and carefully graded to embrace many of the problems of projection.

The plates are made to face the descriptive text. There are many inaccuracies in the text, but perhaps no more than usually results from attempts to simplify scientific rules so as to render them acceptable to learners who are not conversant with standard text books.

The book will be found useful by mechanics who wish to begin mechanical drawing by themselves, but in its present form it can hardly be recommended for schools.

DANGERS TO HEALTH. A PICTORIAL GUIDE TO DOMESTIC SANITARY DEFECTS. By T. PRIDGIN TEALE, M. A. New York: D. Appleton & Co.

The pictorial portion of this book is so elaborate that the text is hardly needed. Seventy full-page plates are introduced, and are devoted mostly to the illustration of defects in plumbing arrangements. In order to exhibit plainly the objectionable air and water currents, they are printed in blue.

The book is designed to be of service to householders, landlords, physicians, architects, and health officers.

It is safe to say that all ordinary sanitary defects are so plainly shown by this work, that the ordinary householder may learn how to make a satisfactory examination of the sanitary arrangements of a dwelling.

ELECTRICITY IN THEORY AND PRACTICE, OR THE ELEMENTS OF ELECTRICAL ENGINEERING. By LIEUT. BRADLEY A. FISKE, U. S. N.

The preparation of this book has resulted from a well-founded conviction, that practical men and students have found great difficulty in seeing the relation between the theory of electricity and its practical application, because they have had to study the theory of electricity from books solely devoted to abstruse theory and the applications from books wholly devoted to practical applications. The author has endeavored to supply a want felt by both classes, by preparing a treatise explaining the principles upon which practice depends.

To accomplish this purpose, a brief outline of the theories of magnetism and electricity is first given; then follows an exposition of the principles of electrical measurement, after which, in order, telegraphy, electric lighting, electric machines, motors, electric distribution of power, meters and electric railways.

The book, doubtless, occupies a position not heretofore filled in scientific literature, and we believe will satisfy the want of a large class who earnestly desire to know the relation between the electrical science of an academic course and the practice of electrical engineers.

THE CHEMISTS' MANUAL. By HENRY A. MOTT, JR., Ph. D., F. C. S.—Second Revised Edition.—New York: D. Van Nostrand.

The estimate of the value of this work to the student of practical chemistry, as expressed by Dr. Chandler, and printed as a preface to the first edition, has been fully justified by the exhaustion of the first supply and a demand for the second.

The revision has included such changes in atomic weights, and the addition of such new methods of analysis as the progress of science demanded.

The work includes treatises on both quantitative and qualitative analysis by both wet and dry methods; also chapters on mineralogy, stoichiometry, and physiological chemistry, together with a miscellaneous collection of tables and formulas.

It is a complete ready reference book for the laboratory worker.

MISCELLANEOUS.

FOR the preparation of cathedral glass, flashed or wholly colored, blown or cast glass plates are, under German patent 22,306, coated with a mixture of equal parts pulverized basalt, potash, saltpetre, and calcined borax made into a paste with water and subjected to a red heat after drying. The temperature must be high enough to fuse the coating and soften the glass simultaneously. The cooling is effected in the usual manner.

THE report of Colonel Frank Bolton, the official metropolis water examiner, for September, again draws attention to the way in which the water purified and filtered with every care by the water companies is polluted all over London by the receptacles which are employed under the intermittent supply system. Dr. Frankland's report shows and states the water supply to be good, and "again unusually free from organic matter." It always is "unusually free" now, we are glad to see.

ON the power required to shear hot steel blooms some figures have been given in *Stahl und Eison* by Mr. R. Lauenstein. The shears with which the experiments were made are driven by a 10 by 16 horizontal engine geared one to four and a-half, the stroke of the shears being 9 in., and the dimensions of the bloom 6¾ in. square. When the engine is running at a speed of 45 revolutions the power is just sufficient to cut the blooms, the speed of the fly wheel being sensibly affected. When the blooms were not quite hot enough the engine stopped without cutting entirely through the whole bloom. This, therefore, proved to be the minimum limit of speed. From this Mr. Lauenstein calculates that the entire pressure upon the cutting tool of the shears was 125,120 lbs. or 2,746 lbs per square inch of the bloom to be cut.

LIGHT STANDARDS.—M. Monnier, directory of the laboratory installed at Paris (by the gas companies) for the study of electricity, has undertaken a comparison of the existing standards of light. He finds the value of the French carcel to be in English standard candles 8.33, in German candles 7.5, in Munich candles 6.5. The standard equivalent values of those in vogue are for the English 9.5, and German 7.6; therefore some change in this respect would seem to be advisable. Schilling's equivalents to the carcel are English 9.6, German 9.8, and Munich 8.7. On the other hand Schilling's measurements give the corresponding values as 7.77, 7.4, 8.7 There is thus room for a correct determination, and on the face of this disagreement between other observers we are the less inclined to trust to M. Monnier's results. It is high time some definite system of light standards was adopted. What is the Committee of the International Electrical Congress appointed to consider this question doing?

COLONEL BOLTON says in his September report on the London water: "It has been suggested that the question of a practical 'standard of quality'—including both the organic and inorganic matter contained in water—should be considered and determined by the highest authorities connected with the medical and chemical professions, and when such standard has been settled by these authorities, it will then become the duties of engineers connected with the water companies to work up to such standard, so that the sources of supply of water to the metropolis may be thereby regulated."

ALLOYS of metals are often difficult to make, and very small quantities greatly affect alloys. The presence of $\frac{1}{5000}$ of a pound of antimony in a pound of melted lead increases the rapidity with which the lead oxidises and burns. Lead which contains more than $\frac{1}{14000}$ of its weight of copper is unfit for the manufacture of white lead. *Der Techniker* says gold with an alloy of $\frac{1}{1000}$ of lead is extremely brittle. Copper with $\frac{1}{2}$ per cent. of iron has only 40 per cent. of the electric conductivity of pure copper. Nickel was regarded as a metal which could be neither rolled, hammered, nor welded, until it was found that the addition of $\frac{1}{1000}$ of magnesium, or of $\frac{3}{1000}$ of phosphorus, makes it malleable. Some varieties of cast steel are exceedingly brittle, but the addition of $\frac{1}{12}$ of 1 per cent. of magnesium makes them malleable. At the Paris Exposition of 1878 a great difference was found in the toughness of sheets which were made of Swedish puddled iron.

IN Dr. Frankland's report to the Registrar-General, on the quality of the water supplied to the metropolis during the month of August, it is stated that the so-called "organic impurity" of the river-derived water was from $2\frac{1}{2}$ to $2\frac{3}{4}$ times as great as that of a particular well-water adopted by himself as a standard. As usual, however, Messrs. William Crookes, William Odling, and C. Meymott Tidy, say it is not pointed out, *per contra*, that the so-called "previous sewage contamination" of the standard well-water is more than twice as great as that of the Thames and Lea derived waters. Neither is it pointed out, though shown by Dr. Frankland's own figures, that the so-called "organic impurity" of the river supply of London is appreciably below that of the Corporation of Birmingham's supply, and also, as we have on several occasions shown, of the highly reputed Loch Katrine water supplied by the Corporation of Glasgow. It is not for us to furnish an explanation of the omissions from an official and presumedly impartial report.

DR. JOULE has been experimenting, with a view to counteracting the bad effects produced by the sulphuric acid, which the combustion of ordinary illuminating gas produces in sufficient quantities to destroy the binding of books and to tarnishing the lettering on their backs, besides, of course, vitiating the atmosphere so much that the health of the person breathing it is injured. He suspended two plates of finely perforated zinc, one 3 in. and the other 12 in. above the burner. At the end of three months the lower plate showed an accumulation of the ordinary brownish-black deposit, and a furring of sulphate of zinc, but the upper plate was only slightly affected. "The inference," *Knowledge* says, "from this examination is that a single plate of perforated zinc, about a foot square, placed over a gas jet is sufficient to retain most of the noxious emanations." Such a thing would be a nice-looking ornament, and it might be suggested that the difficulty could also be overcome by using tallow candles, or, perhaps, an electric light.

PROTECTION OF IRON FROM RUST.—The problem of protecting iron from rust is one of perennial interest, and new systems of painting or otherwise treating iron for this purpose are continually being proposed. It has been observed that iron lying still and exposed to the air, as in railways not in actual use, rusts more quickly than when the metal is strongly vibrated by constant traffic. From this it has been inferred that the vibration is attended with an electrical action that decreases the affinity of the iron for oxygen. In tearing down old masonry, iron clamps and bonds are sometimes met with which, when completely bedded in mortar, are as free from rust as when they left the blacksmith's hands.

A French engineer, says the *Polytech Notizblatt*, observed this remarkable effect when uncovering the anchor plates of several chain bridges which had been built for about thirty years. Where the anchors had been covered with the fat lime mortar of the masonry, they showed no sign of rust; but the parts of plates that had been prolonged into empty space were so rusted that two-thirds of their substance had gone. It has been repeatedly observed that iron does not rust in water in which are dissolved small quantities of caustic alkalies, or alkaline earths, which neutralize every possible trace of acid.

These experiences are apparently the bases of a theory propounded by Herr Riegelmann, of Hanau. The paint that he uses contains caustic alkaline earth (baryta, strontia, &c.), so that the iron is in a condition analogous to that of the anchors of the chain bridges already mentioned. Although a thin coat of paint cannot contain so much alkali as a thick bed of mortar, the alkaline action will nevertheless have effect so long as the coating has a certain consistence. Under any circumstances, these new paints will be free from active acids. Riegelmann's paint, moreover, is said to contain a rust-preventing composition which does not require the aid of any alkali in order to effect its purpose. Perhaps this is the same mixture described in the *Neueste Erfindung*, where it is stated that if 10 per cent. of burnt magnesia, or even baryta or strontia, is mixed cold with ordinary linseed-oil paint, and then enough mineral oil to envelope the alkaline earth, the free acid of the paint will be neutralized, while the iron will be protected by the permanent alkaline action of the paint. Iron to be buried in damp earth may be painted with a mixture of 100 parts of resin (colophony), 25 parts of guttapercha, and 50 parts of paraffin, to which 20 parts of magnesia and some mineral oil have been added.